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THE FUNCTION AND TEST OF DEFINITION AND METHOD IN PSYCHOLOGY[1]

AMID all of the discussion current in the last few years among psychologists the unprejudiced outside observer might think that we were a body of men professing to develop and teach a science who did not know what that science was to deal with and without any idea or with too many ideas as to the methods that should be followed in undertaking to develop our knowledge of the unknown or undetermined subject-matter. Psychology is at once the science of mind, the science of consciousness, the science of experience, the science of behavior. Psychology must be studied only by careful watching of the processes of the individual, by the individual himself; one who does not proceed in this way is no psychologist, no matter how valuable his work may be as physiology or biology or sociology. On the other hand, we are assured by just as devoted and well-recognized psychologists that psychology must deal only with the responses of the individual, with what can be seen from the outside, and that what the first man deals with really has no existence, or at best is entirely irrelevant to the responses, to anything that is of scientific interest. If we are to be taken at our own valuation we are either altogether unfit to carry on the task we have set ourselves or entirely unprepared for it.

As a matter of fact I presume this comes from the youth of the science, at least from taking a definition and formal statements

[1] Address of the Vice-president and Chairman of Section H—Anthropology and Psychology, Philadelphia, December 30, 1914.

of method too seriously. Other sciences have the same trouble with definitions. It would be as difficult to find a single phrase that would mark off physics from chemistry in an absolutely accurate and adequate way as to distinguish psychology from anthropology or human physiology, and quite as difficult to formulate a definition of either chemistry or physics that would satisfy every one, as to define psychology. From most traditional definitions, J. J. Thomson as physicist has no right to be discussing atoms, and similar violations of the sacred rights of physics as defined in the text-books might be cited on the part of men who are generally labeled as chemists. These men, and the better men in the sciences in general, are not interested in phrasing definitions but in solving the problems that their science, or closely related sciences present to them. On our side, much of the discussion, or the liveliness of the discussion, comes from the fact that we assume that the definition must determine the science rather than the science the definition. It is assumed, tacitly, to be sure, that a definition is logically prior to the science, is a statement from which the science may be deduced or a program that the science is to follow in its development rather than a mere statement of what the science has done, or a formulation, as best we may, of the aims common to the mass of workers who are generally accepted as psychologists.

If we are to accept the view that a definition is the servant of our science rather than its master, if we are to say with Judd "that we all know what psychology is," then the test of a definition is that it shall state the aims of the science in the briefest form possible, and in terms that shall be best understood by the individuals for whom it is intended, that shall be least open to misunderstanding. If we consider the traditional definitions we find that each is open to certain objections when tested by these criteria. The traditional science of mind implies a general agreement as to what mind is, and this is lacking. It also suffers from the implication to men who do not know what psychology is that we are to deal with an entity of some sort, for all terms after they become familiar come to be regarded as denoting things. When mind is defined in a way to avoid this implication, as it usually is in the succeeding sentence, it is no longer recognizable by the uninstructed. The same objections hold against consciousness; it was at first innocent enough of mystical significance, but a very few years of use to designate the material to be studied set it up as a thing or inner force. Hypostatization followed close upon the heels of its entrance into definitions. If psychologists were to study it as the material of their science it must be the equivalent of mind as mind is of soul, it must then be an active agent that psychologists can see, although, like the holy grail, sight of it is granted only to the pure in heart. Experience as a substitute for consciousness or mind escapes some of the disadvantages in that it is less likely to be personified or substantialized, but it is difficult accurately to separate the part of experience that psychology is to treat from that touched upon by the other sciences. When this is accomplished it affords little advantage over mind or consciousness.

In view of all these circumstances a change from the inside to the outside, to describe the object of psychology as behavior, offers the most advantages. Behavior is at once simple enough to require the minimum of definition and is hardly capable of being transformed in meaning to designate a thing or force. It takes, too, the attitude toward the mental of the average non-reflective individual. The ordinary

man is interested primarily in the mind of others rather than in his own. He is interested in furnishing stimuli of various sorts to other men that shall lead or compel them to act in certain ways rather than in how he himself or his fellow feels as he acts. The advertiser is content if his copy induces men to buy, the orator if his discourse brings him votes or changes the mind of his audience to his own opinion. The salesman is content with his knowledge of practical psychology if his patter leads the buyer to part with his money, the general or statesman if he can divine how his opponent is likely to act under the conditions he presents to him. In the simplest as in the most complex and important affairs of life the practical man is concerned not with mental states, but with behavior. He usually assumes mental states to account for behavior, but they are purely hypothetical, not the result of introspection, however crude. Good temper and bad temper, conceit and modesty, weak will and strong will, are all names for qualities that can be recognized through behavior alone, or at least can be no more easily recognized through introspection than by observation. The bad-tempered man is as little aware of it and can give as little explanation for it as his friends or enemies. He knows of his weakness only by observation of his actions rather than by any mental process that precedes or accompanies his acts, and is probably, through his prejudices, even less likely to recognize the quality than are others. To turn in upon one's self, to have "too much contemplation in one's eye," is for the average man a sign of weakness, a forerunner of mental disintegration. The mental states of the uninitiated are not known through watching himself, but assumed to explain the behavior of another man.

On the theoretical side, behavior has the advantage over the more subjective terms as a designation of the subject-matter of psychology that it includes many processes that are treated by practically all of us. Very much of the active life bears very little ascertainable relation to consciousness when closely analyzed. It is not putting the matter too strongly to say that the more the voluntary processes are analyzed, the smaller part does consciousness seem to play in them. The less voluntary processes, habit, instinct and the various impulses are also included in the list of psychological processes, although little or no consciousness accompanies them. They are quite as easily predicted from without as from within. Even the learning processes and the recognition processes are studied quite as easily by observation as by introspection. One knows that one recognizes through observation of his mental states, but sees very little of how he recognizes. One can be almost as sure that another has recognized him as he can that he has recognized the other. Neither can determine immediately how the recognition has taken place. Thinking by the most recent workers would be put on much the same level. Even the self or personality, if one is to use the more familiar and objective term is quite as much removed from introspection as from observation. On the whole, if one were compelled to choose between behavior and consciousness as a designation of the subject-matter of psychology and then should apply the term in all logical strictness, it would be found that more of the actual content of the average text-book on human psychology would need to be eliminated if one deleted the portions that applied to consciousness than if one omitted those sections that were devoted to behavior.

If we leave human psychology and turn to animal psychology, no one would deny

that a study of behavior is all that we have aside from an uncertain amount of discussion as to how closely or remotely the human mind can find a parallel behind the actions of the lower forms. Similar is the problem raised by the assertions of the recently prominent group of philosophers who insist that consciousness is non-existent—at the most an illusion. For them psychology as the science of consciousness has ceased to exist. While fashions in philosophy change too often for the psychologist to attempt to square his definitions with all of them, it is nevertheless interesting to see that psychology defined as behavior is quite as applicable to the philosopher without a mind as to the rest of intelligent creation. His responses to stimulation, his perceptions as they modify his actions, his memory and capacity for reasoning with varying degrees of accuracy under different conditions, even the conditions that led to his denying that he was conscious, could be studied with some degree of satisfaction. Antecedent acts and experience could be shown to give rise to the various actions, and would go far toward explaining them.

But it does not follow that because much of the material in the text-books and much that the common mind regards as mental is really a matter of behavior that a definition of psychology as the science of behavior would change the nature of the science. As was asserted in the beginning, the science makes the definition, not definition the science. There is no mental process, however strictly one may follow the subjectivist, that does not have some influence upon behavior. The very description of them in words itself implies behavior. Perception in all forms, images of all types, feelings and emotions, not to mention the mental antecedents of voluntary action, all play a part in determining the character of the individual. Each modifies his behavior. If one understood thoroughly the behavior of any man he would also understand his consciousness. It is possible to neglect behavior in the study of consciousness, but not to neglect consciousness in the study of behavior. The only ones who could object to the statement that behavior was to be understood in terms of consciousness are the men who deny the existence of consciousness, and they need no convincing as to the possibility or even the desirability of defining psychology as the science of behavior. To my mind, the adoption of behavior to designate the subject-matter of psychology need not change in the least the treatment of the subject as ordinarily presented. Even the individual who finds no interest in anything but the classification of his own mental states, if such there be, could go on with his classification, and, if he classified all of his states, would find an awareness of his own movements among them, and find these very important both as the beginning and the end of his series. He would probably prefer another description, but his own work would be included in the definition, he would still be within the pale. By adopting the definition we change our description of the science not the science itself.

It should be added that in the nature of the case no definition can be satisfactory. No single phrase, or paragraph even, is sufficient to definitely delimit the subject-matter of psychology. Even a short text can not include and describe all that might be and is in reality included in the science. The meanings of terms are bound to grow, and with each change a definition becomes inaccurate. Of course, were one to take the other attitude that the definition fixes the science, the difficulty would be avoided. But there is no absolute authority to fix that definition and even if it could be fixed by such an authority the science would soon

find itself on a procrustean bed. Advance would be impossible. If the science is to determine the definition, the statement can be at best a short-hand description of it, it can do no more than approximate either completeness or accuracy. A definition is no more than a choice of evils. All that is incumbent upon us is that of all evils we choose the least.

Even more the subject of conflict at the present moment than the definition of the science is the question of the methods that may be employed in developing it. On this point psychologists have been even more divided and each more strenuous in insisting upon his own attitudes. Whether a new science attracts the more aggressive and in consequence more intolerant men in the scientific community, or the very uncertainty of the subject of method leads to an over-emphasis of assurance, a whistling to keep up courage, or what the psychoanalyst would call an emotion that arises from the constant repression of a complex of doubt that must be kept below the threshold because of its unpleasantness, it is undoubtedly true that psychologists have spent more time than most scientists in insisting upon their own method or the methods that they have adopted. *Ex cathedra* statements, and assertions that all who do not follow their own method are not psychologists and that all who do follow it and reach results that do not conform with their own are not psychologists, have been relatively very frequent. Several instances may be mentioned. Wundt, as you all know, early in the history of the science asserted that no man who could not obtain the sensory and motor differences in reaction times was to be included among psychologists, and only recently after a controversy with Bühler on the *Ausfrage* method he announced that he would read no more reports on work done by that method. At present Müller and Meumann, both respected leaders in the science, are indulging in a controversy in which each seems to fall back upon similar personal criteria as a justification for their impatience with the standpoint of the other. No psychology without introspection has been a motto frequently implied if not explicitly asserted, and, ironically enough, an advocate of the newest method to claim a monopoly turns upon the former tyrant among methods with the assertion that it has been dealing with an illusory material, that the method is worthless, and that its followers have retarded the development of the science and are in general cumberers of the earth. Turn about is fair play, but to meet intolerance with intolerance is usually more interesting and picturesque than helpful to the science.

To my mind the great difficulty on both sides lies in the same tendency that makes trouble with the definition, the method rather than the science is given priority. The method should be the servant of the science, not the science the slave of the method. The only test of a method is its accomplishment. Just as with definition no authority exists that can once and for all say this is the method, follow it or cease to be a psychologist. Attempts on the part of any one to take that tack are quite certain to be a means of covering the uncertainties or the mistakes of the author; they are certain not to be fruitful for the science. These must have their origin in prejudice rather than in any universal law revealed to that individual alone. Any method that gives results must be kept, and the more we have the better. What are to be called results offers room for difference of opinion, but the gradually developed judgment of the recognized members of the science and of related sciences will be the final arbiter of that question. With

the complexity of our subject-matter any method that can give a point of attack is to be encouraged on general principles. The methods that prove fruitless will disappear soon, the valuable ones will assert themselves. Meanwhile a broad hospitality that will encourage originality, rather than a hidebound insistence upon any single method, will certainly be beneficial for the advancement of the science.

That the advocates of a method are prone to exalt the method at the expense of the science, to make over the science if not the man to conform to the needs of their method, can be seen to-day in the writings of both introspectionists and behaviorists. The introspectionists in general desire to put all the essential mental operations on the inside, to find them in images, while Watson, their newest and most vigorous opponent, would put all on the outside. Thus in the thought processes the more thoroughgoing believers in images insist that thinking that does not go on in images is not thinking, or that the individuals who announce that they do not use images have overlooked their images through faulty observation of some sort. They themselves heap up images for each of the reasoning operations, in spite of the fact that many of the processes they mention are obviously individual if not irrelevant to the end that is accomplished. Watson, similarly, after announcing that psychology is a branch of behaviorism and its method is the observation of external responses under experimental conditions, feels himself compelled to transfer the thinking process in its entirety to the outside where the experimenter can discover all that goes on. Thinking must be found in contractions of the larynx, in slight movements of the larynx, or in other movements at present undiscovered which must however lie upon the surface of the body. It is not apparent why he should insist on the slight movements of the larynx, for which delicate apparatus should be used, rather than upon the full movements of speech which may be even more completely analyzed by the ear. If the language of the individual does not tell us why he reaches certain conclusions when he thinks aloud, I can not see how the slight movements made when he thinks to himself are to be of any greater aid. So far as any evidence on the subject exists, the movements in thinking are but faint replicas of the movements of ordinary speech.

If we take the thinking process as an instance, I am inclined to believe that the great difficulty is not so much with the method as with the way in which it is applied. Advocates of both tend to deal too much with irrelevant materials. As an impartial onlooker I am convinced that much of the imagery that we hear so much of in the long introspective accounts is wholly or largely irrelevant to the problems, and I am sure if I may indulge for a moment in the cocksureness that I am criticizing, that the slight recorded movements that are mentioned on the other side would be at most irrelevant accompaniments, rather than essential conditions of the thought process. If one observe any bit of thinking as revealed in the speech of another or in one's own consciousness, if one happen to have a consciousness, it is seen that there is no difficulty in knowing that a conclusion has been reached and in deciding that it is or is not adequate. How the conclusion is reached, and why it seems adequate or inadequate, is revealed neither to introspection nor to observation. To answer either of these questions one must proceed as one would in the natural sciences by varying the antecedents of each process until one discovers that certain are the real causes and others are chance accompaniments. If experimentation is not

possible, study of the conditions under which the conclusions are reached and of the way the conclusion varies with the immediately preceding events and with earlier experience may give the same result. Heaping up descriptions of accompanying imagery or of accompanying movements may be of no more value to this end than is collecting postage stamps in the study of the causes of events in the world's history. Both may be interesting as mementoes, but throw no light upon underlying causes.

In the list of irrelevancies in connection with the reasoning processes is the question whether one may think without images so much under discussion at the present. Proof that men may think without images is a valuable advance, not in itself, but in so far as it raises the question how he really does think. If two men reach the same conclusion, one with, the other without images, obviously the presence or absence of imagery is equally unessential. The only alternative is to believe that the one man has images, but does not notice them, or that the other thinks in spite of his images. That one thinks and how is the essential, and the individual with the imagery is no more and no less effective in attaining conclusions than the one without. They are equally accurate, and neither knows directly how he accomplishes his results. The quarrel over the nature of the mental state has obscured the more important problems of reasoning. For this reason it seems to me that the important accomplishment of the Würzburg school has been not to prove that thinking may go on without images, although I am prepared to accept that, too, but to show that the antecedent purpose, the *Aufgabe*, determines the course of thought. One shows what is not needed, the other an element that is essential.

From this standpoint the attempt to set up a new element of pure thought rather than to study the actual operations of thinking is unfortunate. All that has been shown by introspection is that images are lacking, not that anything else is present. To assume pure thought is to hypostatize our ignorance. Particularly objectionable is this because no attempts have been made to determine its conditions, to set limits to it, or to reduce it to any law. It is merely another addition to our collection of postage stamps, perhaps even less valuable than the others because denomination and name of the country have been worn off, and no one knows what the remnants of the portrait mean. The great disadvantage with the introduction of the term, is that, as with all names, in the course of a few years all problems of thought, all reasoning operations, will be explained by reference to it. If one asks why John reasons better than Jane the answer will be that John possesses more of the pure thought element. To be sure, none of the advocates of the new school mean anything of the kind at present, and it may never develop in this way, but the tendency to use these more or less mystic entities in mystic ways is strong. A word becomes a thing on the slightest provocation.

By asserting that conscious states may be irrelevant, it is not implied that they are always or even usually irrelevant; in fact, in opposition to Watson it seems to me that many mental states are relevant and that one knows what goes on in mind quite as well or better from the inside than from the outside. Not only does the study of imagery indicate its existence in all but relatively few individuals, but Meumann's and many other studies indicate that it has an important influence upon the method and capacity of an individual's learning, his spelling, the methods of mental calculation and many other activities. To take

a concrete instance, if I may be indulged for a mention of names: Here are Yerkes and Watson who have been working approximately the same time with the same problems and materials and attaining the same conclusions in their chosen field. But recently, when they came to the application of methods to human psychology, one makes much of imagery and of introspection in general, as much as the most ardent introspectionist could wish, while the other denies the existence of imagery except for the sake of argument in a few sporadic cases. If one assume the attitude of the average man and argue from behavior to consciousness, it is evident that while Yerkes has a large amount of concrete imagery, probably dominantly visual, Watson has relatively little concrete imagery, and what he has is of the motor type. An assumption of this sort on the basis of behavior alone, if we are to include writing psychological treatises under behavior, is of course not to be compared in value with a few minutes' introspection, but may be ventured as a guess. If this holds, not only is consciousness and even imagery an essential determinant of behavior, but it is possible to show that one important bit of the behavior of the man who would most emphatically deny the existence of imagery is due to imagery or its lack. To ascertain that one does not have imagery is just as much a contribution of introspection as to determine that it exists. To give over introspection altogether is to abandon the method that has given much if not most of the body of knowledge that we have at present and to insist that we use only a method that so far has been little tried, and which, in the form that is suggested, the inference of mental states from slight movements, has when tested proved relatively futile.

If one broaden slightly the term consciousness and the implication of introspection it seems possible to put the problem of psychology in a form that removes all ground for complaint on all sides. This is to include in consciousness and among ideas the fundamental states upon which all effective mental life depends. More immediate than the image, more certainly made out than any slight movement, is the series of assurances that we have that certain events, subjective or objective, take place. We know that we recall, we are sure that we recognize, believe, see objects, that we are pleased, desire certain things, and are on the point of striving for them. These assurances are common to the man who has images and to the man who has none, to the man who believes that mental life is fundamentally sensory and to him who regards it as altogether motor, to the realist and to the idealist. They might be called mental states, or mental functions, had not both been spoiled by use. It is the mind of the practical man before he does any theorizing. It is likewise the starting point of the psychologist. He begins to deal with images and with slight movements only when he becomes sophisticated, and when he becomes sophisticated he forgets his starting point and substitutes his explanation of consciousness in terms of images, movements, or pure thought for the fundamental reality. In time he assumes that the explanation instead of the fact is the reality, just as the naïve man assumes that memory, attention, will and self are immediately known realities.

My plea is that the real subject-matter of psychology is the fact that we attain conclusions, that we perceive distance, that we are prepared to act, rather than the imagery, or the movements that accompany, precede or succeed. This group of facts common to all schools may be explained in different ways or need not be explained at

all. It is always possible to determine the laws of any mental operation, as has been done by the experiments on memory, by the statistical method applied to everything from heredity to advertising by the Cattell school and others, by the investigators in education, in medicine, in the studies in efficiency, and also in the early experiments on Weber's law and reaction times. Whether classed as conscious processes or as behavior, every one is capable of deciding whether a sound is more intense than the preceding, whether a picture is more or less beautiful than another, of recalling and recording the words that were spoken in a conversation a month ago (whether truly or not is for the experimenter to decide) of pressing a key when a stimulus is given. That is all that these experiments require. How these processes are carried out is entirely indifferent. Granting that they may be carried out, a science of psychology is possible. All disagreement between schools is as to how these judgments are made, that they are made all agree.

This conclusion does not mean that psychology need stop here. How one remembers, the mental antecedents of an act and all questions of classification and of ultimate explanation are bound to be raised and are at once valuable and interesting—my only contention is that the nature of the explanation offered makes no difference to the fact to be explained, a statement that is obvious enough but which seems to be lost sight of in much of the controversy that is raging. The laws that I have been mentioning correspond to the simple physical laws of the lever, of gravitation, Ohm's law and Joule's law, etc., while the controversy rages about questions related to the physicist's discussion of the nature of ether and the atom and the so-called law of relativity. Whether one is to use introspection or observation as the method of psychology arises only when one seeks an explanation of mental laws, not while discovering them. For this explanation introspection, observation and speculation on the basis of both and of knowledge obtained from all related fields can, I believe, all be used to advantage. No one method is complete in itself; in most experiments all three are used, no matter to what school the investigator belongs.

One may take as an instance such an experiment as those of Ach on action with the reaction time method. The fundamental result, let us say, is to determine that the response that follows, showing two numbers written one over the other, depends for its character and the time required upon the purpose. That fact is independent of the method used. If one is interested in the antecedents of the movement in consciousness, one must introspect. But raw products of introspection are valueless. One must be assured that the images are essential by repetition of the introspection with the same individual and other individuals under varying conditions. To determine the nature of the purpose and the way it acts one may see if it has any conscious form, and may indulge in physiological speculations, may look for analogies in physiological laws, or one may observe the bodily attitude, the set of the muscles before and during the response. The final acceptance of any explanation will be found to depend upon a harmony of all these observations with each other and with related facts. In any case, the determination of the laws is related to their explanation as observed fact is related to theory in physics or physiology.

The question might well be raised whether the certainty of recognition, of decision and the other processes we have mentioned as constituting the primary facts of

the mental life, are the products of introspection or observation. On this point there is room for difference of opinion. A large number of the processes, recognition, judgment, feeling, seem to be more closely related to introspection; the active processes, on the other hand, the comparison of divided with accumulated repetitions and perceptions, are either derived from observation or a combination of observation and introspection. In addition to these immediately observed, generally recognized mental states and functions there are immediate facts derived both by introspection and by observation aided by experiment. Such are on the one side the awareness of the different sorts of imagery, the course of association, colored hearing and the different synesthesias, and, on the other, the changes in circulation with mental operations, the slight movements, and the larger movements of expression. These and many other immediate facts of consciousness escape the untrained observer or introspector, but are needed to round out the series of mental facts and to aid in the formulation of expansions of other facts and laws.

In brief then there is room in psychology for the greatest variety of standpoints and for all methods, provided only the spirit of live and let live prevails. The science is above the individual and the individual's preference in definition and method. The definition and method in turn must grow out of the science; they are not given once and for all, and the science forced into them. Given a set of facts and laws of fairly general acceptance, the form of statement again is largely a matter of individual preference guided and tested by the interest and comprehension of the group for whom the discussion is intended. As in most sciences a mixture of explanation and theory with bare fact may be used, or bare facts may be stated and explanation follow or be omitted. Methods that are assumed by the investigators may be with advantage followed in the restatement of their results. But formulation of results and their presentation in a treatise can no more be determined by a priori principles than can the statement of definitions or the prescription of methods. In brief, my plea is for the widest liberty in all respects with a testing of everything by results rather than by formulæ or even by tradition. In the light of the tests so far available it seems to me that defining psychology as the science of behavior and the use of all methods possible under suitable precautions will lead soonest to the end of psychology, the discovery of mental laws and their explanation.

And we have no reason to be ashamed of the progress of the science. More has been done in the discovery of fundamental laws in the last sixty years than in all the preceding centuries from Thales to Fechner, and interesting problems open to our methods of approach on every hand. These laws, the immediate results of experiment, are not in dispute. They have stood the test of repeated investigation, and are accepted on all sides. There is much more difference of opinion about theories, but even here we have made progress. Except for the fact that we still take our theories very seriously, even our theories offer no more occasion for controversy than do theories on similar problems among physiologists, or zoologists or much more than between physicists and chemists.

W. B. Pillsbury

Mrs. Henry Draper

Anna Palmer Draper, widow of Dr. Henry Draper, died on December 8, 1914, at her home in New York City. Her name will always be honorably associated with the science of astro-

physics. It is interesting to note that the wives of two of the men connected with the beginnings of this science played such important parts in the careers of their husbands. Sir William Huggins, who first applied the spectroscope to the stars, had in his wife, the talented Margaret Lindsay, an enthusiastic and capable co-worker during many years of incessant labor. Dr. Draper was also fortunate when, in 1867, he married Mary Anna, the gifted daughter of Courtlandt Palmer, of New York City. For Mrs. Draper not only was her husband's associate in his investigations during the fifteen years of their lives together, but after his early death in 1882, she was able to provide for carrying on his work in a most efficient manner.

It is said that Dr. Draper became especially interested in astronomy in 1857, while attending the meeting of the British Association in Dublin. He was invited by the Earl of Rosse to go with a party to Birr Castle, Parsonstown, to see the famous six-foot reflector. So great was the impression made upon Dr. Draper by this giant telescope that he resolved to construct a similar, although smaller, one for himself. This he did, and in 1867 a reflector of 28-inch diameter was placed in his private observatory at Hastings-on-Hudson. In the summer, Dr. and Mrs. Draper resided at Dobbs Ferry, two miles distant, and it was their custom to drive together to the observatory for the evening work. So great was her interest that he never went to the observatory without her, and in the days of the wet plate, she herself always coated the glass with the collodion. Mrs. Draper told how sometimes after they had been to the observatory and returned to Dobbs Ferry on account of clouds, they would find the sky clearing, and would drive back again two miles to the observatory and recommence work. During the early years of their married life, Dr. Draper was experimenting with the photographs of stellar spectra with his reflector, and in May and August, 1872, he succeeded in photographing the spectrum of Vega, showing four dark lines. This was four years before Huggins obtained a photograph of the dark lines in the spectrum of this star. In 1878, Dr. Draper organized an expedition to go to Rawlins, Wyoming, for the purpose of observing the total solar eclipse of July 29. Mrs. Draper not only went with him, but also assisted in various ways. Her special duty was to count the seconds during the eclipse and lest the vision might unnerve her, she was put within a tent and therefore saw nothing at all of the wonderful phenomenon. Here she sat patiently and accurately calling out the seconds while the glorious and awe-inspiring spectacle was unfolded. Some of us remember her among those gathered on the roof of Hotel Monticello in Norfolk, Virginia, on May 28, 1900, when without instruments we merely observed the total eclipse for its beauty and grandeur. What memories it must have recalled to her of the distant western land where nearly a quarter of a century before she sat inside the tent and called out the seconds for her distinguished husband!

In the winter, Dr. and Mrs. Draper resided on Madison Avenue, New York City. Here he established a laboratory, connected with the residence by a covered passageway, where his work not dependent on the telescope could be carried on, and where his photographs could be studied. The house, which is between Thirty-ninth and Fortieth Streets, is spacious and well adapted to elaborate entertaining. When originally built by Mr. Palmer, it was the last house in New York City, and he was cautioned by his business friends against investing in property so far away from the center. Mrs. Draper remembered when the old omnibus running on Fifth Avenue went only as far as Thirty-ninth Street, so that when any one alighted and started to walk in their direction they were sure of a visitor.

In November, 1882, when the National Academy of Sciences was meeting in New York City, Dr. and Mrs. Draper entertained the members at a dinner said to have been one of the most brilliant ever given there. As a novelty, Dr. Draper lighted the table with Edison incandescent lights, some of which were immersed in bowls of water. About fifty were present, and at the close of the dinner, Dr. Draper, although suffering from a severe cold,

moved about and talked with several of the guests, among others, Professor E. C. Pickering, director of the Harvard Observatory. They discussed in particular the photographs of stellar spectra Dr. Draper had obtained. Professor Pickering expressed to Dr. Draper his great interest in that work and offered to measure these photographs if they could be sent to Cambridge. Almost immediately after the dinner Dr. Draper was seized with a congestive chill, followed by pneumonia which proved fatal a few days later.

Mrs. Draper, who was in deep distress after this sudden loss, desired to establish some memorial to her husband, and for a few years contemplated the erection of an observatory in New York City. This plan proved impracticable, however, and in 1885, she visited the Harvard Observatory, where Professor Pickering was already photographing stellar spectra along the same line as the work which she considered the most important her husband had done. She thereupon decided to found the memorial in connection with the Harvard Observatory, and gave generous sums each year for its prosecution. At first she thought only of continuing the researches on stellar spectra, but in 1887 she decided to extend the plan to include all available facts about the constitution of the stars. She not only gave liberally of her means to carry on this work, but she always took a great personal interest in it. Until deterred by failing health she visited Harvard Observatory regularly, and personally inspected the progress of the work, giving advice about matters of policy, and being greatly interested in the actual inspection of various stellar spectra. All peculiar or new types were submitted to her, and she often exclaimed with girlish eagerness, "How interesting it must be to do it!"

Mrs. Draper was a friend to many scientific men and frequently gave elaborate entertainments in her spacious home. The old laboratory in New York was fitted up as a lecture or exhibition room and could seat two hundred people. Here many famous men came to lecture to scientific societies and invited guests. Here various scientific exhibitions were placed when she entertained such societies as the National Academy or the American Astronomical Society. It is quite unusual for women of wealth to entertain in this manner. Few who have such beautiful homes, have such a desire or interest.

The results of the Henry Draper Memorial have been varied. The first catalogue giving the spectra of a large number of stars was published in 1890, and was called the Draper Catalogue. This contained 10,351 spectra. Following closely upon this came detailed discussions of about 5,000 spectra of the brighter northern and southern stars. In 1911, observations were commenced for a New Draper Catalogue, which will contain the spectra of at least 200,000 stars situated over the entire sky. In this work Mrs. Draper was greatly interested until the very last, and wrote encouragingly about its progress.

In the course of the Draper Memorial work, various discoveries have been made, such as 10 novæ, more than 300 variable stars, 59 gaseous nebulæ, 91 stars of Class O, and a large number of peculiar spectra. Among the greatest results may be mentioned, the establishment of the true order of stellar evolution, and such discoveries as the connection between variability and changes in spectra, the additional series of hydrogen lines, and the existence of spectroscopic binaries.

Who can predict to what further uses the great collection of plates will be put or what further increase in our knowledge of the sidereal universe will be made by means of the generous endowment left in memory of Henry Draper by his devoted and noble wife.

Annie J. Cannon
Harvard College Observatory,
Cambridge, Mass.

A NEW GLACIAL PARK

Announcement has been made through the press of the gift to the New York State Museum of a plot of ground covering seventy-five acres which includes the remarkable Green Lake near Jamesville, N. Y., with its series of abandoned cataracts, rock channels and dry plunge-basins. This spot is not only extremely

picturesque, but now that the significance of its singular conformation has been pretty well worked out by the labors of E. C. Quereau, and more particularly by Professor H. L. Fairchild, it constitutes a very extraordinary, if not unique, geological record.

In the course of Professor Fairchild's work upon the Pleistocene geology of New York state, he demonstrated very clearly and in detail the accuracy of Mr. Quereau's suggestion that in the retreat of the ice mantle the outflow of the glacial waters was by way of tremendous rivers moving eastward into the Mohawk-Hudson drainage, and here one of these streams cut its rock gorge in the limestones of the Helderberg escarpment and left a series of plunge-basins beneath great cataracts which surpassed the dimensions, as they must have equalled the dignity and grandeur, of Niagara.

The Green Lake or Jamesville Lake, which lies on the property now thus reserved, is surrounded on all but its eastern side by an amphitheater of sheer limestone cliffs rising to a height of nearly 200 feet, and the depth of the lake is stated by the former owner of the property to be not less than 100 feet. While water still fills this ancient plunge-basin, it is water of a deep emerald hue, without visible outlet or inlet. Westward of this escarpment is a smaller and dry plunge-basin with its abandoned cataract cliff and with rocky channels connecting it with the larger basin, and from the Green Lake eastward is the old open discharge into the other stream courses and cataracts lying beyond Jamesville in the vicinity of Fayetteville.

Aside from the extraordinarily clear and wonderfully effective geological record displayed in this place, the spot has additional scientific interest as its rocks are the resort of many rare ferns and flowering plants which have long attracted the botanist.

The menace of commerce, expressed in the ever-increasing demand for the conversion of limestone into cement, threatened this wonderful spot, and the intervention of the donor, who saved it from destruction, is a particularly gracious act inasmuch as it conserves a place of high scientific and educational interest.

The property is given to the regents of the university for the State Museum by Mrs. Mary Clark Thompson, of New York, and presented in the name of her father, Myron H. Clark, a former governor of that state, and by her desire it is to be known as the "Clark Reservation."

It may be added that this reservation lies about four miles to the southeast of Syracuse on the Seneca Turnpike, a new state road, and is also easily accessible from Jamesville which can be reached from Syracuse by trolley.

JOHN M. CLARKE

ALBANY, N. Y.,
March 3, 1915

THE UNITED STATES GEOLOGICAL SURVEY AT THE PANAMA EXPOSITION

THE exhibit occupies a space 62 by 78 feet in the Palace of Mines and Metallurgy, flanked on one side by the exhibit of the Bureau of Mines and on another by the Alaskan exhibit, for which also the survey has been in a measure responsible. The central feature of the exhibit is a booth, containing stage-like settings of a scene, partly modeled and partly painted. The first represents an undeveloped district in the arid west being studied by the survey. Topographers are at work with their instruments on the headlands; geologists have stripped a bed of coal and are taking a sample for analysis; and other geologists are studying the rocks. In the foreground is an automatic gage beside the river that comes out of the picture toward the observer. Farther back, a stream gager is measuring the stream. In the background is a camp and pack train. The second scene shows the same district after development. The results of the stream gaging have been utilized in planning a power plant that shows in the distance and an irrigation project that covers the valley floor. The coal bed is being mined on one side; an oil field is under development elsewhere; a sandstone bed is being quarried in the foreground; mining and milling are in progress in the mountains; a town has been built, and roads, railroads, and other evidences of civilization abound.

Behind the scenes, in the same booth but

facing the ends, are recessed screens, on one of which are shown pictures illustrating the different kinds of survey work and the part they play in the development of the country. On the other screen are shown several series of pictures.

At one end of the space is shown the per capita production of minerals in the United States in 1880, about the time of the Centennial Exposition, and of the organization of the survey, and in 1913, the period between these dates practically covering the past work of the United States Geological Survey. The exhibit consists of one 97-millionth of the actual production of each mineral in 1913 and one 48-millionth of the production in 1880.

The space along one of the outside aisles is devoted to a series of cases, illustrating what our common things are made of, what the raw material looks like as it is obtained from the earth, and where it occurs in the United States. For example, many of the familiar household articles are there, such as an albuminum saucepan, an electric-bulb filament, and a fountain-pen point; and above each article is shown the mineral from which it is made, traced back to the ore, and then a map of the United States, showing where the ores occur. Most of these individual maps have been prepared especially for this exhibit.

At the west end of the space is an exhibit of the power and fuel resources of the United States, including maps showing the distribution of the black shale from which oil is derived and the apparatus used in the field in determining the shales that are worth studying.

In order to show the transparencies included in the exhibit to the best advantage, arcades resembling mine entrances have been built at the corners of the space. The methods of work in the survey are illustrated by a series of cases showing by a set of partial results how maps are made and other features of the work.

In the portion of the exhibit relating to water resources is a display of automatic gages being run by clock work and recording the fluctuating height of water in a tank.

One feature of the exhibit is the stereoscopic pictures, resembling the old mutoscope views but of a modernized type. These will be arranged in boxes of fifty each on a table at which one may sit and study leisurely various features of survey work. There are also shown four series of pictures of the Grand Canyon and Rocky Mountain region, taken in the early days of the geological survey by the famous photographers Jackson and Hillers.

Other cases show the gem minerals, the rare mineral ores, etc.

SCIENTIFIC NOTES AND NEWS

Col. George W. Goethals has been made a major-general of the line in recognition of his services in building the Panama Canal. Brig.-Gen. William C. Gorgas, surgeon-general, has been made major-general in the medical department. Col. Harry F. Hodges and Lieut.-Col. William L. Sibert, United States Corps of Engineers, have been promoted to be brigadier-generals. The bill providing for their promotions extended the thanks of congress to the officers.

Under the leadership of Dr. Hiram Bingham, the National Geographic Society-Yale University Peruvian Expedition sailed from New York on March 3 to continue its work in the Andean Mountains. Members who left New York on this expedition are: Director, Hiram Bingham, Yale University; geologist, Herbert E. Gregory, Ph.D., Silliman professor of geology in Yale University, geologist of the 1912 expedition; naturalist, Edmund Heller, naturalist of the Smithsonian's African expedition, under the leadership of Colonel Roosevelt; botanist, O. F. Cook, Ph.D., of the United States Department of Agriculture; chief engineer, Ellwood C. Erdis, of the 1912 expedition; topographer, Edwin L. Anderson; chief assistant and interpreter, Osgood Hardy, M.A., of the 1912 expedition; assistant topographer, C. F. Westerberg, B.S., and several assistants.

Miss Katharine Lilly, head nurse of the department of surgery of the Rockefeller Institute for Medical Research, has gone to

France to assist Dr. Alexis Carrel, of the institute, who recently has been detached from the Lyons Hospital and placed in charge of a hospital at Compiègne, France, near the northern line of battle. Dr. H. D. Dakin, the biological chemist, who worked some years in this country, has also joined Dr. Carrel.

PRESIDENT RAYMOND A. PEARSON, of the State Agricultural College at Ames, Iowa, has decided not to accept the offer of Governor Whitman, of New York, to become state commissioner of agriculture to succeed Mr. Calvin Hudson. Dr. Pearson was commissioner of agriculture under Governor Hughes.

MR. WILLET M. HAYES, formerly assistant secretary of agriculture, has returned from a year's service as adviser to the government of the Argentine Republic and of the Province of Tucuman.

DR. and Mrs. N. L. Britton, of the New York Botanical Garden, Mrs. N. Wille, Mr. John F. Cowell, director of the Buffalo Botanical Garden, and Mr. Stewardson Brown, of the Philadelphia Academy of Natural Science, are in Porto Rico engaged in botanical explorations.

DR. JANET T. HOWELL, daughter of Dr. William Howell, professor of physiology in the Johns Hopkins Medical School, has been awarded the Sarah Berliner Fellowship for Women. This fellowship carries with it a gift of $1,000 to enable the recipient to engage in research work in physics, chemistry or biology. Dr. Howell received the A.B. from Bryn Mawr College in 1910, and the Ph.D. from the Johns Hopkins University in 1913. She was holder of the Helen Schaffer Huff research fellowship in physics at Bryn Mawr College during 1913–14 and this year she holds the position of lecturer in physics at Bryn Mawr College, taking the place of Professor James Barnes.

ON February 26, Professor Alexander Smith, of Columbia University, delivered a lecture to the Boylston Chemical Club of Harvard University on "The Forms of Sulphur and Their Relations."

PROFESSOR W. K. HATT, of Purdue University, lectured at the University of Illinois on February 24 on the subject of "Flood Protection in Indiana."

IRA O. BAKER, professor of civil engineering in the University of Illinois, lectured recently before the students of the Short Course in Highway Engineering at the University of Michigan. His subject was "Selecting the Road Surface."

DR. WALTER HOUGH, curator of ethnology, U. S. National Museum, gave an address before the California Academy of Sciences on February 17, on "Explorations of a Sacred Cave in Arizona."

DR. BARTON W. EVERMANN, director of the Museum of the California Academy of Sciences, gave the Sigma Xi lecture at the University of California on February 24. His subject was "The Conservation of the California Elk."

DR. RICHARD MILLS PEARCE, professor of research medicine in the University of Pennsylvania, addressed the Buffalo Academy of Medicine on Wednesday evening, February 24, on "Experimental Studies of the Spleen in its Relation to Anemia, Hemolysis and Hemolytic Jaundice." A reception to the speaker followed the lecture.

WE learn from *Nature* that M. Louis Moissan, son of the late Professor Henri Moissan, and assistant at the Ecole supérieure de Pharmacie at Paris, who died on the field of battle on August 10, has left to his school, in addition to the scientific books and apparatus of his father, the capital sum of 200,000 francs for the foundation of two prizes—one for chemistry (prix Moissan), and one for pharmacy (prix Lugan), in memory respectively of his father and his mother, *née* Lugan.

DR. T. WESLEY MILLS, emeritus professor of physiology in McGill University, died in London on February 14.

PROFESSOR JAMES GEIKIE, the distinguished geologist, died in Edinburgh, on March 2, in his seventy-sixth year. He entered the British Geological Survey in 1861 and was called to the Murchison chair of geology at Edinburgh University in 1882, succeeding his brother, Sir Archibald Geikie.

Dr. Richard Weitzenböck, aged thirty years, docent for chemistry at Gratz, has been killed in the war.

A despatch from Rome states that all physicians in Vienna who are under fifty years of age have been ordered by an imperial decree to join the army medical corps.

The Rockefeller Institute for Medical Research has appropriated $20,000 to be used under the institute's direction to further medical research work under war conditions, and is equipping Dr. Carrel's new hospital in France with apparatus for research work on pathological, bacteriological, surgical and chemical conditions.

The New England Association of Chemistry Teachers held its fifty-second regular meeting on February 27, at the Roxbury Latin School, when an address entitled "Some possible items, new and old, for the course in elementary chemistry," was given by Professor Alexander Smith, head of the department of chemistry in Columbia University. At the request of the executive committee Professor Smith discussed several topics, such as: Action of air in the Bunsen burner flame; colloidal suspensions; cause of valence, electrons; the shortest route to atomic weights; the distinction between physical and chemical change; and new view of a crystalline solid. Several experiments were performed to illustrate these subjects. The members who were present in large numbers discussed the value of these topics in an elementary course in chemistry.

The U. S. Civil Service Commission announces an examination for metallographist, for men only, to fill a vacancy in this position for service in the Engineering Experiment Station, Naval Academy, Annapolis, Maryland, at a salary of $2,500. The duties of this position will be (*a*) to direct the preparation of metal specimens for microscopic examination and the photographing of the same, and to interpret the appearance of specimens under the microscope; (*b*) to prescribe correct heat treatment for steel specimens which have not had proper treatment; (*c*) to make and interpret the various standard physical tests applied to metal specimens; (*d*) to investigate miscellaneous problems that may arise in the course of naval practise, such as the cracking of the tin linings of copper cooking kettles, imperfect welds, various processes of galvanizing, etc.; (*e*) to investigate the properties of various alloys of metal; (*f*) occasionally to make a chemical analysis of metallic substances. The degree of Ph.D. from a college or university of recognized standing, and at least five years' experience since receiving the bachelor's degree, such experience to have included the use of the microscope in the examination of metals, and the making and interpretation of photomicrographs of metals, are prerequisites for consideration for this position.

A systematic study of Missouri River and its tributaries is being carried on by the United States Geological Survey. Considering the varied character of the streams of the Missouri River basin and their great economic value for irrigation, power, and other purposes, the investigation is one of the highest importance. The water supply of this great drainage area is the subject of a publication recently issued by the Geological Survey, entitled "Surface Water Supply of the Missouri River Basin, 1912" (Water-Supply Paper 326), by W. A. Lamb, Robert Follansbee, and H. D. Padgett. This report contains the records of flow at 130 permanent stations of the survey during the year 1912, data which are necessary to every form of water development, whether it be water power, navigation, irrigation, or domestic water supply. Some of the tributary streams are exceedingly variable in flow; others, like the Niobrara in Nebraska, are remarkably uniform. The Missouri proper is formed in southwestern Montana by the junction of three streams which were discovered by Lewis and Clark in 1806 and were named by them Jefferson, Madison and Gallatin rivers. Of these three Jefferson River drains the largest area and is considered the continuation of the main stream. This part of Montana is mountainous and affords many excellent water-power sites. Among the principal tributaries of the Missouri are the Marias, Musselshell, Yellowstone, Cheyenne,

Platte and Kansas. The western part of the basin is in the arid belt and the eastern part is in the semiarid and humid regions. Ten states of the Union are drained in part by Missouri River. Rising at the Red Rock Lakes, at an elevation of 6,700 feet above sea level, this stream descends through the Rocky Mountains and emerges on the broad prairie land a few miles below the city of Great Falls, Montana. From that point it is accounted a navigable stream with an easy grade, and in passing through the Dakotas and along the borders of Nebraska, Kansas and Iowa it receives the flow of great tributaries, so that as it crosses the State of Missouri and joins the Mississippi a short distance above St. Louis it becomes one of the large rivers of the world. Its total drainage area is about 492,000 square miles in extent and comprises, in addition to the states above mentioned, large areas in Wyoming and Colorado and a smaller area in the southwestern part of Minnesota.

THE Michigan College of Mines has received a collection of minerals from the Shattuck Cave, near Bisbee, Arizona, one of the wonders of the mining world. This cave was opened in 1913 by a drift on the third level of the Shattuck Mine. When the miner who had been drifting in this part of the level returned one night after a heavy blast, he found that the working face had entirely disappeared and that before him was a great opening reaching farther than his light would shine. Looking upward he could see tiny lights flashing and believing that they were stars he ran back to the shaft, declaring that he had blasted a hole clear through to surface. Mine officials investigated at once and found that a great natural cavern had been opened up, circular in shape, 340 feet in diameter and 175 feet high. It was a virtual fairyland of beauty, myriads of crystals in the roof reflecting back the lights from the miners' lamps. Walls, roof and floor were covered with great clusters of crystals, and near the center of the cavern a cluster of stalactites hung from the ceiling in the form of a great chandelier 40 feet long. The crystals were for the most part pure white, but in places where the filtering waters had contained iron and copper, the beauty was enhanced by great transparent stalactites and stalagmites, some ruby red, others a clear emerald green or azure blue. The mining company illuminated the cave with electricity and has allowed thousands of visitors the privilege of seeing it. An attempt was made to have the Smithsonian Institution at Washington remove and reproduce a portion of the cave, but nothing came of it. It is because the mine operators have now found it necessary to fill the cave with waste rock that the Shattuck-Arizona Mining Company sent the specimens to the College of Mines. Superintendent Arthur Houle, of the Shattuck Company, is a brother of Professor A. J. Houle of the college.

UNIVERSITY AND EDUCATIONAL NEWS

THE Massachusetts committee on education voted unanimously on February 25 in favor of "taking initial steps toward the establishment of a state university."

ROBERT FLERSHEIM has left a bequest of a million marks to the University of Frankfurt.

DR. FRANK J. GOODNOW will be formally inaugurated president of the Johns Hopkins University on or about May 20. It is planned to give the occasion a double significance in inaugurating the third president of the university and formally dedicating the new site at Homewood.

AT Smith College the following promotions have been made: from assistant professor to associate professor, Inez Whipple Wilder, A.M., department of zoology; from instructor to assistant professor, Mary Murray Hopkins, A.M., department of astronomy, and Grace Neal Dolson, Ph.D., department of philosophy.

THE senate of the University of London has conferred, as we learn from *Nature*, the titles of professor and reader in the university upon the following: Dr. A. L. Bowley (London School of Economics), statistics; Mr. L. R. Dicksee (London School of Economics), accounting and business organization; Mr. J. E. S. Frazer (St. Mary's Hospital Medical School), anatomy; Dr. T. M. Lowry (Guy's

Hospital Medical School), chemistry; Mr. J. H. Morgan (University College and the London School of Economics), constitutional law; Dr. W. J. R. Simpson (King's College), hygiene and public health; Mr. J. H. Thomas (University College), sculpture; and Mr. G. Wallas (London School of Economics), political science.

DISCUSSION AND CORRESPONDENCE

HEADSHIP AND ORGANIZATION OF CLINICAL DEPARTMENTS OF FIRST-CLASS MEDICAL SCHOOLS

To the Editor of Science: In the October 30, 1914, number of Science there is a very interesting and timely article by Dr. Meltzer, of the Rockefeller Institute, on the reorganization of clinical teaching in this country, "Headship and Organization of Clinical Departments of First-class Medical Schools." The subject is a very important one and I feel sure that it will interest the many medical men who have the opportunity of reading your journal. Dr. Meltzer refers in his letter, which is written to a university president, to the report of the Council on Medical Education of the American Medical Association made to the House of Delegates of the A. M. A. in June of last year. He takes occasion to criticize in his letter several statements made in this report, and especially the statement "that the medical school very properly demands that its clinical teachers be men who are recognized as authorities in their special fields, both by the profession and the community," and he further objects to the use of the term "grotesque" as referred to a plan in which it is proposed that clinical teachers may do private practise, but that fees from such practise are to be turned into the university treasury. He also questions in advance the value of a report on the reorganization of clinical teaching that is to be made by a committee of the well-known clinical teachers to whom this subject has been referred by the Council on Medical Education.

As chairman of the Council on Medical Education I am very glad that this important subject is being discussed in the columns of such an influential journal as Science and by such an able physician and research worker as Dr. Meltzer. I feel, however, that the readers of Science and college presidents and trustees could not form an accurate view of the position taken by the American Medical Association from Dr. Meltzer's letter alone, and without reading the portion of the report of the Council on Medical Education referring to this subject, and therefore am enclosing this special part of our report from page 15 to page 17.

In the reorganization of our medical schools one of the most pressing needs is that of placing the clinical departments on a more satisfactory basis. Little has as yet been done in this country with this problem, and the time has arrived when the medical profession and the medical schools must take up this matter vigorously and formulate a general plan of organization of our clinical departments and urge its adoption. With this in view the Council on Medical Education has appointed a strong committee of ten clinicians, who have had great experience in teaching and who are regarded as authorities in their special departments and in medical education, to study this subject and report to the conference on medical education.

The organization of a clinical department is a more complex subject than that of a department like anatomy, or physiology, where teaching and research are the functions demanded.

In clinical work the head of the department and his associates must be three things; first, great physicians in their special field; second, trained teachers; and third, research workers. The medical school very properly demands that their clinical teachers be men who are recognized as authorities in their special fields, both by the profession and by the community. In the organization of a clinical department this fact must not be lost sight of and whatever plan is adopted must make it possible for the clinical teachers to remain the great authorities in their special fields both in the eyes of the profession and of the public.

The plan adopted by the German universities has been on the whole most satisfactory. There a professor in a clinical department is in every sense a university professor just as

much as the professor of chemistry or of physics. His university work commands his time. He must allow nothing to interfere with his teaching, his clinical work in the hospital or his research, and he devotes on the average quite as much time to his university work as does his colleague in chemistry or in mathematics. In addition to this, however, he devotes some time each day to private practise by which he maintains his position before the profession and the public as a great specialist. This can be done without neglecting his university position. In fact, if he does not remain the great physician, he ceases to be of as much value either to his students or to his university. On the other hand, if he should neglect his university work because of the time he devoted to private practise, his services would be dispensed with.

This problem of clinical teaching has been taken up during the year by the General Education Board and, as a result, an interesting experiment is to be tried at Johns Hopkins and possibly at one or two other places. The General Education Board has given Johns Hopkins $1,500,000 endowment with which to pay salaries to the departments of medicine, surgery and pediatrics. The position is taken in this experiment that the head of a clinical department should be given a very large salary and should receive no fees for private practise. It was recognized at once that the rich should not be deprived of the services of these experts, so the grotesque plan is proposed that these men may do private practise, but that fees from that practise are to be turned into the university treasury and not into their own pockets. [As will be seen by the context the word "grotesque" does not apply to the plan as a whole but is used to characterize that part of it which proposes that these clinical teachers may do private practise but are not permitted to receive any fees for these services, the understanding being that the fees are to be assessed and collected and appropriated by the university or hospital. I desire to assume the full responsibility for this particular portion of the report and to submit that the term "grotesque" is an exceedingly mild one to characterize such an unethical and illegal scheme. That the fees for the peculiarly individual and personal service rendered by a physician or surgeon to his patient should be appropriated by any institution and not go direct to the medical man rendering such service is clearly unethical. It is equally clear that it is illegal, as the institution would have no standing whatever in court if it sought to collect for itself the fees for such service. It is interesting to note in this connection that although these propositions are perfectly clear to men who are practising medicine, they are not as self-evident to non-clinical and non-medical men who are not in a position to understand the rights and interests of the medical profession.]

The men who proposed this plan, and provided the money necessary to make the experiment, are non-medical men; they do not have the medical point of view and they do not understand the complex functions demanded of the clinical teacher.

This plan has not been well received by the clinical teachers and finds its supporters almost entirely among the laboratory men. It is difficult to understand if the teachers in a medical school are to be placed on salaries and not permitted to receive any compensation for outside work, why the clinical teacher should be given a very large salary and his colleague in anatomy or in pathology a comparatively small one. The sweating of the scientific men who have devoted their lives to teaching and research on miserable salaries is notorious. Advantage has been taken of the fact that their scientific enthusiasm would hold them to their work and they are often as underpaid, comparatively, as the workers in a sweat shop. Surely, if the medical department of a university receives large endowments for the payment of salaries, the men teaching in the laboratory sciences should receive the first consideration. Again, if a clinical department obtained large sums for salaries, why should they pay a very large salary to the head of the department who in a very limited amount of time devoted to practise could obtain for his services much more than the amount of such

salary? Would it not be better to devote the available money to paying several younger men from 25 to 35 years of age—their more productive years—to devote practically all their time to teaching and research? Fortunately for the plan, the men who are responsible for it recognize that it is an experiment and frankly advise that it be not adopted by other medical colleges until it has been tried out on Hopkins.

The fact that such a plan has been seriously proposed by laymen interested in education emphasizes the necessity of a thorough reorganization of our scheme of clinical teaching along lines to be determined and agreed on by a committee of our best clinical teachers.

I should like to add the following comment.

First, that the Council of Medical Education believes that one of the most pressing needs is that of the reorganization of our clinical departments on a more satisfactory basis.

Second, the Council of Medical Education has taken the position that this important subject of the reorganization of clinical teaching should be submitted to a committee of experts, men who are recognized as great clinical teachers and who are familiar with the problems of clinical medicine. This committee is composed of the following men: Dr. V. C. Vaughan, University of Michigan, President of the American Medical Association; Dr. Geo. Armstrong, McGill University; Dr. John Finney, Johns Hopkins University; Dr. John Clark, University of Pennsylvania; Dr. W. J. Mayo, trustee of the University of Minnesota; Dr. Geo. deSchweinitz, University of Pennsylvania; Dr. Frank Billings, Rush Medical College, University of Chicago; Dr. Harvey Cushing, Harvard University; Dr. Geo. Dock, Washington University, and Dr. Saml. Lambert, Columbia University.

The committee is at present working on this problem. The Council on Medical Education does not know as yet what the findings of this committee will be. We believe, however, that the report of this committee will be of greater value than would the report on this particular subject of a committee of university presidents, professors in the science departments of universities, professors of the laboratory branches such as embryology, chemistry or physiology in a medical school, or men who are devoting their lives to the problems of medical research, but who are not in touch with clinical medicine. Would it not be well for university presidents, university trustees and medical faculties who contemplate reorganizing their clinical departments to await the findings of this committee representing the American Medical Association? The subject was discussed February 16, 1915, at the annual conference on medical education held in Chicago and the final report will be made to the house of delegates at the June meeting of the American Medical Association.

Arthur Dean Bevan

SOIL NITRATES

To the Editor of Science: In the reviews of an article[1] by Mr. Wright and myself appearing in a monthly bulletin of the International Institute of Agriculture[2] and the *Chemical Abstracts* of the American Chemical Society,[3] the point of view supported by our paper is not fully recognized. One review refers to the malnutrition of citrus trees as resulting from the toxic effects of superabundant nitrates, and the other refers especially to the production of malnutrition from the denitrification of soil nitrates. We presented the data of our experimental studies in California in some detail in order to draw attention to what we believe to be an important phenomenon, namely, that probably identical symptoms of malnutrition result either from superabundant nitrates which we regard as one phase of the so-called "alkali" poisoning and by nitrogen starvation which may re-

[1] "Relation of Bacterial Transformations of Soil Nitrogen to Nutrition of Citrus Plants," Kellerman, K. F., and Wright, R. C. (Bureau of Plant Industry, U. S. Dept. of Agr.) in *Journal of Agricultural Research*, Vol. II., No. 2, p. 101–13, Washington, D. C., May, 1914.

[2] *Monthly Bulletin of Agricultural Intelligence and Plant Diseases*, Year V., No. 9, p. 1166, September, 1914.

[3] *Chemical Abstracts*, Vol. 8, No. 15, p. 2769, August 10, 1914.

sult either from a natural property of soil nitrogen or may be caused by denitrification of soil nitrates usually caused by improper cultural methods.

K. F. KELLERMAN

SCIENTIFIC BOOKS

Water Reptiles of the Past and Present. By SAMUEL WENDELL WILLISTON, professor of paleontology in the University of Chicago. University of Chicago Press, 1914. Pp. vii + 251, with 131 text-figures.

This interesting volume summarizes in a most authoritative manner our knowledge of the reptiles which have become adapted to aquatic life, and it also includes a chapter on the classification of reptiles, a subject upon which Professor Williston, with his forty years of special study, is abundantly fitted to speak.

In his introduction the author speaks earnestly in the defense of reptiles, which are so often of ill repute as cold, gliding, treacherous and venomous creatures shunning sunlight and always ready to poison. As a matter of fact, but few reptiles possess these evil propensities, for, aside from the venomous serpents, there are but two poisonous reptiles known, and the vast majority are not only innocent of all offense toward man, but are often useful to him. More than four thousand reptiles are living, representing, however, but four of the fifteen orders which were formerly alive. The terse definition of a reptile as a cold-blooded, backboned animal which breathes air throughout life is not surely correct, since it has been believed that certain extinct ones may have been warm-blooded.

While there are very marked distinctions of structure between the amphibians and the reptiles, there can be no doubt that the early amphibian ancestors of the modern toads, frogs and salamanders were also the ancestors of all living and extinct reptiles. This is proved by the fact that discoveries of recent years have bridged over nearly all the essential differences between the two classes so completely that many forms can not be classified unless one has their nearly complete skeletons. In the case of some of the oldest amphibia, the Stegocephalians, we know that they were water-breathers during part of their lives, because distinct impressions of their gills have been preserved, but we are not so sure that some of the more highly developed kinds were not air-breathers from the time they left the egg; if this be true, our definition of a reptile as distinct from an amphibian is rendered still less secure. We are quite certain that from some of the early extinct reptiles—probably the immediate forebears of the great dinosaurs—the class of birds arose, while another group of primitive reptiles, called the Theriodontia, and known chiefly from Africa, gave rise to the mammals.

The classification of reptiles is still a matter of much doubt and uncertainty, no two authors agreeing on the number of orders or the rank of many forms. Many strange and unclassifiable types which have come to light in North America, South Africa and Europe have thrown doubt on all previous classification schemes and have weakened our faith in all attempts to trace out the genealogies of the reptilian orders; and classification is merely genealogy. It is only the paleontologist who is competent to express opinions concerning the larger principles of classification of organisms and especially the classification of reptiles. The neozoologist, ignorant of extinct forms, can only hazard guesses and conjectures as to the relationships of the larger groups, for he has only the specialized or decadent remnants of past faunas upon which to base his opinions.

Williston's scheme of classification differs only in minor details from the more conservative of the generally accepted views, and those differences are, for the most part, the writer's own opinions, to be taken for what they are worth. It may be said decisively that no classification of the reptiles into major groups, into superfamilies or subclasses that has so far been proposed is worthy of acceptance; there is no such subclass as the Diapsida or Synapsida, for instance.

Williston recognizes and briefly diagnoses fifteen orders, of which three groups, the Proganosauria, Protorosauria and Thalattosauria, are provisionally given this rank.

The third chapter is an illuminating discussion of the skeleton of reptiles, in which the principal elements are not only fully described, but illustrated by the author's drawings.

The chapter on the Age of Reptiles contains a chart showing the range in time of the various reptilian suborders, beginning with the Carboniferous. Each important horizon is taken up in turn and the character of the sedimentation and location of the chief exposures discussed. This section is illustrated by Williston's restorations of various Permo-Carboniferous reptiles.

All this is by way of preparation for the main theme of the book—that of the adaptation of reptiles to aquatic life—and the fifth chapter discusses the problem in general, with the principal structural changes which water-living brings about, comparing the reptiles in their modification with other important aquatic types. Then in regular sequence the water-inhabiting orders are discussed: the Sauropterygia, *Lystriosaurus* among the Anomodontia, the Ichthyosauria in which the summit of aquatic adaptation is reached, *Mesosaurus* of the Proganosauria, *Pleurosaurus* of the Prolorosauria, many of the Squamata, especially the marine iguana *Amblyrhynchus* of the Galapagos Islands, and the Agailosaurs and Mosasaurs, our knowledge of the last named being largely due to the author's own researches.

Another chapter treats of the Thalattosauria recently described by Dr. J. C. Merriam, while the Rhynchocephalia are represented by *Champsosaurus*. Crocodile-like forms are included under two orders, Parasuchia and Crocodilia, *Geosaurus*, an Upper Jurassic crocodile, going to the extreme and developing an ichthyosaur-like tail for swimming. The final chapter treats of the Chelonia, the most sharply distinguished order of reptiles and the one which had the most uniformly continuous and uneventful history from the Triassic to the present time.

This book is a thoughtful exposition of the entire subject from a master hand, and while necessarily technical in part, is written in such a style as to be eminently readable. It departs from the great majority of popular books of "ancient monsters" because it is written by one who has a world-wide reputation as an authority on the subject of which he treats.

In view of the success of the present volume and of the preceding "*American Permian Vertebrates*," Williston's announced volume on the evolution of the reptiles is anxiously awaited.

Richard S. Lull

Yale University,
February 1, 1915

Festschrift Max Bauer zum siebzigsten Geburtstag gewidmet. Edited by R. Brauns. Stuttgart, E. Schweizerbartsche Verlagsbuchhandlung, 1914. Pp. viii + 568, portrait, 32 plates and 47 text-figures; Neues Jahrbuch für Mineralogie, Geologie und Paläontologie, Beilage Band XXXIX.

As a richly deserved and most fitting tribute of regard and esteem to a scientist of distinguished merit, the recent issue of a supplementary volume of the "Neues Jahrbuch für Mineralogie, Geologie und Paläontologie," in honor of the seventieth anniversary of the birth of Herrn Geheimrath Professor Dr. Max Bauer, founder of the Mineralogical Institute of the University of Marburg, enlists the sympathies of all interested in scientific progress, more especially in the domain of mineralogy. This handsome octavo volume extends to nearly 600 pages, and is embellished with an excellent likeness of Professor Bauer, as well as with 32 well-executed plates and 47 text-figures illustrating the subject-matter of the various articles. The dedication from his friends, coworkers and students gives warm expression to their appreciation of his services in the cause of science.

Of the many valuable papers in this testimonial "Festschrift" we can do little more than cite the titles or indicate the subject-matter. Professor Alfred Bergeot, of Königsberg, treats of the structure of the manganese deposits at Meggen-an-der-Lenne, and emphasizes their value in a determination of the geological formation of this region (pp. 1–63);

Dr. R. Brauns studies the scapolite-bearing eruptive rocks in the lake region of Laach (pp. 79–125). A full and interesting paper on the discovery of a small crystal-grotto in pegmatitic granite of Wildenau, Saxon Vogtland (pp. 126–185, 5 plates and 4 figs.), is contributed by Dr. Ross Bruno. A study, with analyses of the basalts of Marburg, is offered by Professor Arthur Schwantke (pp. 531–567, 8 plates and 5 figs.), and one on the origin of talc deposits by Professor C. Doelter of Vienna (pp. 521–530). Much interesting information as to the nephrite of Harzburg is supplied by Dr. J. Uhlig, of Bonn.

There is also a paper on the monazite of Dattas, Diamantina, State of Minas Geraes, Brazil, by Dr. K. Busz (pp. 482–499); the methods employed for investigating the molecular structure of silicate solutions are treated of by Dr. H. E. Boeke (pp. 64–78) and a brief account of a polishing apparatus for crystal planes is given by Dr. Victor Goldschmidt, of Heidelberg (pp. 186–192). This is followed by a paper on an instance of the deposition of sodium in contact-metamorphosis at Langesundsfjord, Norway, by V. M. Goldschmidt, of Kristiania (pp. 193–224), and a study of certain aspects of monohydrate lithium-sulphates, by Dr. A. Johnsen (pp. 500–520). The elæolite-syenite-laccolite of the Sierra de Monachique in southern Portugal is the subject chosen by Dr. Erich Keiser, of Giessen (pp. 225–267, with geological sketch map on Plate XII); Dr. Eduard Raphael Liesegang treats of pseudoclase (pp. 268–276); the nomenclature of crystal forms is studied by Dr. L. Milch (pp. 277–289), and a paper on the granites of the Carpathians is contributed by Dr. J. Morozewicz (pp. 290–345).

In his account of the tin deposits of Tasmania (pp. 346–387) Dr. Fritz Noething expresses the opinion that these deposits are either entirely or in great part of marine, not of fluviatile origin. This paper is followed by one on colloidal silicates by Dr. F. Rinne (pp. 388–414). Besides his study of the nephrite of Harzburg, Dr. J. Uhlig reports on a diopside with manganese from the same region (pp. 446–449). Less strictly scientific, but extremely interesting as a contribution to the historic description of a celebrated volcano, is the paper by Dr. K. Sapper on the "Hell of Masaya," presenting a synopsis of the accounts of this Nicaraguan volcano given by the early Spanish visitors to this region, several of whom ascended to the mouth of the crater, beginning with the ascent made in 1529 by Gonzalo Fernandez de Oviedo y Valdés.

The mere recital of the many subjects treated of in this volume is a sufficient indication of its wide range and of the variety and value of its contents, rendering it an important contribution to mineralogical science.

The birthplace of Professor Bauer was the village of Gnadenthal, near Schwabisch-Hall, in Wurtemberg; at the date of his birth, September 13, 1844, his father was the pastor of the community. Two years later, Pastor Bauer was transferred to Aalau and then to Kunzelsau and Weinberg; in this last-named place he was acting as "superintendent" at the time of his death in 1872.[1]

Max Hermann Bauer was the eldest of eight children, six of whom still survive. In 1859, when fifteen years old, he entered the Polytechnic school in Stuttgart, where he devoted himself to the study of mining engineering and metallurgy, his interest in this latter branch having been awakened by frequent visits to the large iron foundries and mines of Wasseralfingen quite near Aalau, with whose officials his parents were well acquainted. However, the question soon arose whether this field would offer sufficient guarantees for the future in the case of one without private fortune. Of the extremely conservative ideas prevailing in his immediate neighborhood Dr. Bauer writes: "In my native place at that time people usually felt themselves confined to the narrow limits of their birthplace; the idea of seeking elsewhere the more favorable opportunities lacking there, was regarded as eccentric."

Hence the young student, after successfully passing his examination in the Stuttgart Polytechnicum in 1862, turned his attention more especially to mathematics and the natural sciences, which he pursued from 1863 to 1865 at

[1] These autobiographical details were very kindly furnished to me by Professor Bauer.

the University of Tübingen, with the intention of qualifying himself for instruction. When this course was completed, he became an instructor, successively, in several Realschulen in Wurtemberg. His services in this capacity were so highly appreciated that the government sent him to Paris in 1867 to perfect himself in the French language.

However, his growing interest in mineralogy and geology gradually induced him to devote more and more of his time to these studies, and the doctor's degree was awarded him by Tübingen University for a dissertation entitled: "Die Braunsteingänge von Neuenberg." Another period of study in Tübingen in 1868 enabled him to profit by the instruction of Professor Eduard Reusch, an authority on crystallography, and for a time Dr. Bauer confined himself essentially to this science, while not neglecting geology and paleontology. Some of the results of his crystallographic investigations were presented in his first treatise, on mica, issued in 1869, while a number of articles on geology and paleontology as well as his participation for a score of years in the preparation of the special geological map of Prussia, on a scale of 1:25,000, showed his proficiency in these sciences.

The autumn of 1868 found Dr. Bauer in Berlin pursuing a course of study in the mineralogical institute there under the direction of Gustav Rose. The outbreak of the Franco-Prussian War in July, 1870, interrupted these studies, as Dr. Bauer volunteered in defence of his Fatherland. After the termination of this war he was active for a time in Munich and then again in Tübingen, proceeding thence to Göttingen, where he became privat docent of mineralogy and geology. In 1872 he occupied a similar position in Berlin University, also becoming first assistant in the mineralogical institute. From Berlin, in 1875, he was called to the University of Königsberg as ordinary professor of mineralogy and geology. These studies had for a time been somewhat neglected in this university, and there was a sad lack of instruments, books and specimens; not a single fossil was to be seen. This condition of things was entirely changed by Professor Bauer during the one and a half years of his stay. While his lectures embraced the entire field of mineralogy, geology and paleontology, his own personal studies were especially concerned with crystallography and crystallographico-physical investigations. It was toward the close of this residence in Königsberg that he issued his "Lehrbuch der Mineralogie" (1st ed., 1886; 2d ed., 1904).

In the autumn of 1884 Professor Bauer transferred his activities to the University of Marburg, where he has been professor of mineralogy and petrography for the past thirty years. During this period he has devoted special attention to the diabases of Hesse and Nassau and to the basaltic region of Hesse, formations which theretofore had been little studied.

At the time of Professor Bauer's removal to Marburg, he succeeded to C. Klein as editor of the mineralogical section of the *Neues Jahrbuch für Mineralogie, Geologie und Paläontologie* and has carried on this task up to the present time. During this long period more than fifty regular volumes of the *Jahrbuch* and thirty-eight supplementary volumes have been issued, and also more than a dozen volumes of the *Centralblatt für Mineralogie, Geologie und Paläontologie,* a publication connected with the *Jahrbuch,* so that the editorial supervision of Professor Bauer has covered some one hundred volumes of these journals.

The valuable researches and publications of Professor Bauer on precious stones also belong to this period, visits to the famous gem-cutting establishments of Idar-Oberstein and to the diamond-cutters of Hanau having aroused his interest in this direction. A more definite direction to his activity was, however, provided by the plan of a German publishing house to bring out a translation of the present writer's "Gems and Precious Stones of North America." The task of translating and adapting this book was entrusted to Professor Bauer. As the publishers wished to enlarge the scope of the work while retaining its rich embellishment of colored plates, the enterprise resulted in the production of the "Edelsteinkunde" (1st ed., 1896; 2d ed., 1909), an English version of which was published by Dr. L. F.

Spencer, director of the mineralogical department of the British Museum, in 1904.

GEORGE F. KUNZ

Optic Projection. Principles, Installation and Use of the Magic Lantern, Projection Microscope, Reflecting Lantern and Moving Picture Machine. By SIMON HENRY GAGE and HENRY PHELPS GAGE. The Comstock Press, Ithaca. 1914. Pp. 731. $3.00.

Professor Gage and his son, Dr. Gage, have written a timely and compendious treatment of optical projection that will be heartily welcomed by all who are interested in the subject. Such recent developments of the art of projection as cinematography and opaque projection are discussed at length, while the older ordinary forms of projection are not neglected.

The titles of the fifteen chapters are, in order: Magic Lantern with Direct Circuit; Magic Lantern with Alternating Current; Magic Lantern for Use on the House Electric Lighting System; Magic Lantern with the Lime Light; Magic Lantern with Petroleum Lamp, with Gas, Acetylene and Alcohol Lamps; Magic Lantern with Sunlight, Heliostats; Projection of Images of Opaque Objects; Preparation of Lantern Slides; The Projection Microscope; Drawing and Photography with Projection Apparatus; Moving Pictures; Projection Rooms and Screens; Electric Currents and their Measurement, Arc Lamps, Wiring and Control, Candle Power of Arc Lamps for Projection; Optics of Projection; Uses of Projection in Physics, Normal and Defective Vision. In addition there is given a historical outline of the origin and development of projection apparatus, a list of manufacturers of and dealers in projection apparatus, a bibliography and an index of both names and subjects. There are 413 cuts and diagrams.

The authors state that their aim has been to explain the underlying principles upon which the art of projection depends and to give such simple and explicit directions that any intelligent person can succeed in all the fields of projection. The point of view throughout is that of the skilled amateur. To the professional operator the treatment will appear academic, to the theorist it will appear very practical, but all will agree that it covers the middle ground clearly and exhaustively.

P. G. NUTTING

THE METEROLOGY OF ADELIE LAND, ANTARCTICA

THE climatic facts set forth by Sir Douglas Mawson in his interesting volumes, "The Home of the Blizzard," reviewed last week in SCIENCE, justify his claim that it is the stormiest spot on the face of the earth. Although the data as to the weather are desultory and incomplete, except as to the winds, yet a brief survey of this newly discovered land is of scientific interest. Fortunately the expedition was equipped with recording instruments for barometer, sunshine, temperature, wind, etc., so that data exist for full and satisfactory discussion of local meteorology in the promised scientific volumes. Observations were made at the main base, Commonwealth Bay, 67° S., 133° E., and by the sledging parties through King George Land.

No table of monthly means of any kind are given, but it is stated that the mean temperature for the first year was slightly above zero. This is an exceedingly low temperature for the latitude, 67° S. It is, however, not a local cold of radiation, but a cold of translation through the continuous and violent downflow of air from the elevated plateaus of Antarctica, 11,000 feet or more above sea level. The sharp pitch of the land is shown by the rise of 1,900 feet in fourteen and a half miles from the sea. The temperatures were never exceeding low, but were steadily maintained. The minimum temperature at the seacoast was only —28°, and the lowest observed on the ice-cap of the hinterland during the spring sledging was —35°; on September 18, 1912.

From a shaft excavated in the nevé of the hinterland, at an elevation of 2,900 feet, Bage calculated that the mean temperature of the snow, which would be higher than the air, for the year was approximately —16°. It would not be unreasonable from these data to place the mean annual temperature of the south-polar plateau at —40°. The contrast between temperatures during high winds and in pe-

riods of comparative calm are noticeable. On November 19, during good weather, at an elevation of 2,600 feet the temperature fluctuated between zero and 18°, but five days later with a wind of 40 miles per hour it sank to —10°. On December 18, at 5,500 feet, the temperature rose with fine weather twenty-four degrees in a day, while the black bulb registered 105° in the sun.

Wild's station, Queen Mary Land, 66° 30′ S., 95° E., about 1,200 miles to the westward of Mawson's on Adelie Land, appears from the few data available to be somewhat warmer, although the extremes were greater, a mimimum of —38° being reported on December 21, 1912. The monthly mean temperature for June, 1912, was —14.5°, and for July —1.5°, while the means for the German expedition, under Drygalski, about 150 miles to the westward, in 66° 2′ S., 89° 38′ E., in 1902 were 0.5° for June, —0.6° for July, and for a year 11.3°. From these comparative data the annual mean at Wild's station, Queen Mary Land, would be about 8°. Field observations on the glacier-covered hinterland show a minimum of —47°, and a reading of 87° on December 21, from a thermometer laid on an area of black rock. These data probably give an approximation to the annual mean temperature of slightly above zero, Fahrenheit, along the antarctic circle for say 2,000 miles, between 86° and 150° E. longitude.

Bearing on the intimate local relations between the winds and the temperatures of Adelie Land, Mawson says:

> The stronger the wind blew, the less variation did the thermometer show. Over a period of several days there might be a range of only four or five degrees. . . . The compression of the atmosphere during the gusts affected the air temperature so considerably that, coincident with their passages, the mercury column would be seen rising and falling through several degrees.

The only statement available as to the barometer reading runs:

> On July 11, 1913, there was an exceptionally low barometer at 27.794 inches. At the same time the wind ran riot once more—298 miles in three hours. The barometric curve, remarkably even, did not show as much range as one twentieth of an inch. The highest barometric reading was on September 3, 30.4 inches, and the comparison indicates a wide range for a station at sea-level. . . .

Annual barometric means in other portions of the Antarctic regions are as follows: *Discovery,* 77° 51′ S., 167° E., 1902–04, 29.29 inches; Cape Adare, Victoria Land, 2 months only, 29.134; *Belgica,* 70° 30′ S., 88° 30′ W., 1902–03, 29.307; *Gauss,* 66° 2′ S., 89° 38′ E., 1902–03, 29.134. The data of Mawson's expedition will have a bearing on the theory quite steadily advanced, but which the writer has been inclined to question, of a marked anticyclonic area over the vicinity of the South Pole.

The extreme violence of the winds, and the general prevalence of drifting snow have made it impossible to measure with any degree of definiteness the snowfall of Adelie Land. Heavy falls of snow occurred, one being mentioned as amounting in a day to two feet. Of the effect of the wind on the snow Mawson says:

> First, under the flail of the incessant wind, a crust would form, never strong enough to bear a man. Next day the crust would be etched, and small flakes and pellets would be carried away. Long shallow concavities would now be scooped out; these became deeper hour by hour, becoming at last the troughs between the crests of the snow-waves or sastrugi.

The abrasive effects of the drifting snow were astonishing. He adds:

> The southern, windward faces of exposed rocks were on the whole smooth and rounded; the leeward faces were rougher and more disintegrated. On the windward side the harder portions of the non-homogeneous rocks were raised in relief.

Of quantities he says:

> Day by day deluges of drift streamed by the Hut, at times so dense as to obscure objects three feet away, until it seemed as if the atmosphere were almost solid snow.

Mawson adds:

> A point which struck me was the enormous amount of cold communicated to the sea by billions of tons of low-temperature snow thrown upon

its surface, the water already at the freezing point.

The most remarkable feature of the climatic conditions of Adelie Land are the violence and constancy of the winds. They are hurricane in force and, from the data in these volumes, appear to have come invariably from the south-south-east. Their regularity was most remarkable, and the direction so constant that field parties traveled during blizzards and in semi-darkness by shaping their course relative to the wind. Indeed the wind—and the sastrugi formed by it—was a far better direction-guide than was the compass, affected by their proximity to the magnetic pole. The average hourly velocity of the wind for the first year—determined by a registering Robinson anemometer, was fifty miles. The average for March, 1912, was 49; April, 51.5, and May, 60.7 miles. Hourly velocities of 90 miles were not uncommon, and in a number of cases the rate exceeded 100 miles. The most remarkable winds—which from the snow carried by them assumed the character of blizzards—are as follows: 1912 (for the 24 hours), May 11, 80 miles; May 15, 90; May 22 (gust approximating 200 miles per hour, with temperature of —28°); 1913, May, 17 (24 hours), 83 miles; May 18, 93.7 (between 6 and 7 P.M. of the 18th the instrument recorded 103 miles); July 5, 116 miles in one hour, and an average of 107 miles for eight consecutive hours; July 11, 298 miles in three hours; August 16, 105 miles in an hour. Gusts were determined from time to time by an instrument called a puffometer, by which winds in gust were noted of an extreme velocity of about 220 miles, though necessarily such record could not be considered as absolutely accurate.

Meteorologists have usually associated whirlwinds with heated or desert regions. Mawson related:

> Whirlwinds of a few yards to a hundred yards or more in diameter which were peculiar to the country. The velocity of the wind in the rotating column being very great, a corresponding lifting power was imparted to it. The lid of a case, weighing more than 300 pounds, was whisked into the air and dropped fifty yards away. An hour afterwards the lid was picked up again, and struck against the rocks with such force that part of it was shivered to pieces.

Regions of calms sometimes obtained in a sheltered locality immediately under hurricane winds. One man working in a fifty mile gale at the Hut, on the upper cliffs, walked down to the harbor ice and suddenly found himself in an area of calm. As compared with the force of winds of the *Discovery*, 77° 51′ S., 167° E., 10.3 miles per hour, the winds of Adelie Land are nearly six times as violent. As to direction the *Discovery* winds as determined from the lower clouds showed 18 per cent. S., 15 S.W. and 15 S.E. At Cape Adare, with 10 per cent. calms, there were 20.4 per cent. winds from the S.E. and 13.9 from the S. These data seem to bring the S.S.E. winds of Adelie Land in harmony with those a few hundred miles to the southeast. The *Gauss*, 66° S., 90° E., was frozen-in a long distance from land so that its winds, 47.8 per cent. from the E., are not directly comparable with those 1,700 to 2,000 miles to the eastward.

It is evident that Mawson is justified in calling Adelie Land the *Home of the Blizzard*, and in claiming that it is the windiest region on the earth at the level of the sea. Meteorologists will look forward with interest to the publication of the full observations with their scientific discussion.

A. W. GREELY

REPORT OF THE COMMITTEE OF THE AMERICAN ASSOCIATION OF ANATOMISTS ON PREMEDICAL WORK IN BIOLOGY

AT the meeting of the American Association of Anatomists in Philadelphia, December, 1913, a committee was named by the president of the association to confer with the zoologists on the subject of work in biology preliminary to the study of medicine.

In accordance with the original motion of the chairman, which led to the appointment of this committee, the following report was submitted to the association December 29, 1914, at the St. Louis meeting:

Your committee was appointed to confer with the zoologists to ascertain what coopera-

tion may be expected toward standardizing work in biology required of students looking forward to the study of medicine; and to formulate the considerations which would seem practical to incorporate in plans for such courses.

The Zoological Society promptly appointed a committee for this conference, and the following questions were discussed, not only with this committee, but with a number of representative members of the Zoological Society. Besides this, published statements of courses and of discussions on this subject were examined.

The following questions seemed to be most important.

Question 1. Is the work given in different colleges in the elementary, general course in biology adapted to satisfy the requirements of premedical training in this subject?

Question 2. Is it possible to so select and standardize the work of the first year in biology in different colleges as to make it uniform, and to include, here, all needed to make it an adequate course?

Question 3. If an ideal course, including sufficient preliminary work, can not be secured within the one-year period advocated, what principles should be urged to govern the planning of the biological work of students looking forward to the study of medicine, so that they will profit most by the training of the first year, and be best prepared to follow this up in special departments of biology more directly related to medicine.

Question 4. What additional work is to be advised, which is not to be obtained in the first year's general course?

Both committees agree that it is of the first importance to urge the selection of only thoroughly trained scientific men as teachers for this work. Such men can be trusted to insist on real scientific methods and to select the best material and treatment to give the beginner a practical introduction and basis for further work.

Beyond this point, however, the committees were unable to proceed. The zoologists suggested that the anatomists should draw up a statement of what they desire the zoologists to do, in preparing students for anatomy. After this has been done, the zoologists are ready to consider how far it is practicable to meet these needs. Several attempts have been made in this direction, and your committee submits the following statement to the association for its approval, and transmission to the zoologists.

At the present time a one-year's course in biology is generally required as a preparation for the work of the medical school. This study of biology must serve as a preparation for medical work in physiology, pathology, bacteriology and parasitology, as well as anatomy, and it may fairly be questioned whether a single college course is adequate for this purpose. The study of botany alone is obviously insufficient, and the domain of zoology is so vast that much care should be exercised in the choice of those phases of the science to be presented to young students. Courses which are primarily experimental and deal with the functions and reactions of animals, although excellent in preparation for the physiological work of the medical school, are not the proper basis for the study of human anatomy. It is the purpose of this report to point out only those features of the college preparation which experience has shown to be desirable, and in fact essential, for the successful study of gross and microscopic anatomy.

No uniform or stereotyped preparatory course is recommended, for it is recognized that every teacher should give special attention to those subjects and groups in which he is particularly interested, and to the knowledge of which he has contributed by his own researches. Success depends in large part upon the ability of the teacher, but the following purposes of instruction should not be forgotten if the preparatory work is to satisfy the requirements of anatomy.

1. Students frequently begin the study of human anatomy with an insufficient knowledge of the lower forms of animal life. The broad knowledge of the various classes of animals and of invertebrate and lower-vertebrate morphology, which was the inspiration of the

great anatomists of the past, is now too often replaced by vague considerations of the method of science and ideals of observation. A return to the study of animals, as objects of interest in themselves, apart from theoretical considerations and possible relations to human society, is therefore recommended. The student should obtain a synoptic knowledge of the animal kingdom, and should be able to classify, in a general way, and to describe the life histories of the common forms of animals, aquatic and terrestrial, which may be collected in his locality. A beginning in such work may well be made by the student independently or perhaps in high-school courses, but such fragmentary and elementary studies should be supplemented by a thorough college course. The first-hand familiarity with animals should serve as the basis for all further work.

2. As a result of the knowledge of genera and species which the student should have obtained directly for himself by studying some group of animals or plants, questions of the origin of species and of the relation of the great classes of animals to one another are inevitably before him as philosophical problems. Collateral reading then becomes as necessary for the biologist as for the man of learning in any other branch of knowledge. Selected works of Lamarck, Darwin, Huxley, Mendel and others should be freely consulted. This literature, which in its influence upon human thought has far outspread the bounds of biology, should not be neglected by the student of zoology, whose particular heritage it is. Since the idea that science can not be read, and that there is no knowledge in books, is often taught as a cardinal principle, it has come about that students of zoology have little knowledge of, or respect for, the writings of the makers of their science.

3. Before beginning the study of human histology, every student may reasonably be expected to be familiar with the use of the microscope and with the simpler methods of preparing specimens for microscopic examination. This technique can be learned in connection with various courses, perhaps the most useful of which is a general study of the cell with a comparative study of the elementary tissues. The maturation of the germ cells and the processes of fertilization and segmentation can not be properly presented in the medical curriculum, and these fundamental biological phenomena should therefore be observed in college courses. The development of the chick, which was studied primarily by physicians to explain the growth of the human embryo, can likewise receive little attention in the medical school. These subjects are all very desirable in themselves, and if studied by laboratory methods, will supply the requisite skill in the use of the microscope.

4. In preparing for human dissection, comparative anatomy should be studied with the same standards of thoroughness which obtain in the dissecting room. The student should learn to dissect rapidly and well, and to record with careful drawings and brief descriptions the forms and relations of the structures which he has disclosed. But such studies are not useful merely for their methods. A knowledge of comparative anatomy, including especially the anatomy of the lower vertebrates, is indispensable for understanding the structure of the human body. For other reasons also, human anatomy must be treated as an advanced study. The state does not provide bodies for dissection in order that untrained students may learn from them those elementary facts, which may be understood equally well by dissecting cats or rabbits. "It is absurd," says President Eliot, "to begin with the human body the practise of dissection." And the value of dissection is so great in relation to both medicine and surgery, that an adequate preparation should be required. For the study of anatomy, in the words of Lord Macauley, "is not a mere question of science; it is not the unprofitable exercise of an ingenious mind; it is a question between health and sickness, between ease and torment, between life and death."

5. Finally, these recommendations may be summarized as a plea for a more thorough study of zoology on the part of those planning

to enter the medical schools. The zoological courses should not be abridged and popularized in order that time may be saved for other pursuits, or that the science may seem more attractive to college youth. Courses in anatomy and physiology which duplicate the work of the medical school, and courses in "medical zoology," ought not to be substituted for the strictly zoological university courses. The science of zoology is of such great service to students of medicine that it deserves a large place in their undergraduate studies. With medical anatomy, it constitutes "a subject essentially one and indivisible"; and the penalty for its neglect is inadequate preparation for medical practise.

Committee: H. McE. Knower, *Chairman,*
F. T. Lewis,
W. H. Lewis

St. Louis, Missouri,
December 29, 1914

In the following summary, the chairman of the committee has rearranged the main points of the above report in groups, to correspond to the four questions proposed at the beginning; so that a more definite idea may be secured of the manner in which these are answered. In assembling the answers to the different questions the exact sense of the report itself has been retained. In answering questions 3 and 4 an effort has been made to indicate what we may reasonably expect to include in the first year, and what should be advised in addition.

I and II. The first two questions formulated by the committee are answered in the negative; that is, a one-year's course is not regarded as sufficient, and a uniform, standardized course seems undesirable. An introduction to the subject through special courses in selected "medical zoology" is also disapproved.

III. (*a*) In regard to the third question; it has seemed necessary to urge a more thorough knowledge of the morphology of lower forms of animals and their life histories. While the anatomists in adopting this statement as given in the report, undoubtedly expect the physiological aspects of these mechanisms to be considered as necessary accompaniments of such first-hand familiarity with animals, it is urged in the report that the introductory college course shall not be "primarily physiological." It is earnestly desired that the work shall involve a rigorous grounding in comparative morphology, especially of lower forms, which furnishes not only the best basis for human anatomy, but is a very essential preliminary for comparative and human physiology.

(*b*) It is urged that the theoretical and philosophical considerations which accompany the course shall follow a practical acquaintance with animals, rather than that special animal structures shall serve chiefly as illustrative material for lectures on general biological theories, with a neglect of a thorough study of a series of animal forms.

(*c*) The additional principles which should govern the planning of the introductory courses, beyond those just stated, are:

The selection of suitable teachers.

The undesirability of attempting to establish a uniform preparatory course, or courses especially limited to applications to medicine.

The acquirement of skill in the use of the microscope, and of correct scientific method of work in connection with the work of the course.

The beginnings of embryology and cytology.

IV. As to the last question, number 4, the report does not attempt to decide what proportion of the recommended preparation for anatomy can be obtained by a student in the first year's course. This must be indicated by the zoologists. It seems evident to a student of present conditions, however, that most of the work desired in cytology and comparative, general histology; comparative anatomy of vertebrates; or systematic zoology will have to be elected by students looking forward to medicine, after they have taken the introductory course. It is to be hoped that the elements of vertebrate embryology will be included in that course. Some of this work may well be done in one of the excellent summer laboratories.

V. Finally, the importance of collateral reading in the masterpieces of biological literature is strongly emphasized.

At the St. Louis meeting of the American Association of Anatomists, December 29, 1914, the report of the committee on premedical work in biology was approved by the association; and the committee was continued with instructions to submit the approved report to the zoologists, and to secure their cooperation in carrying the work further.

H. McE. KNOWER, *Chairman*

SPECIAL ARTICLES

SEX DETERMINATION AND SEX CONTROL IN GUINEA-PIGS

THE observations, a short exposition of which is given here, were made on guinea-pigs, being used by Professor Stockard in heredity experiments. He very kindly placed the material at my disposal for this study, and I wish to express my appreciation of this favor.

These observations show that the sex of a guinea-pig is determined sometimes by two and sometimes by three factors, depending upon whether the mother has previously born young.

The first factor "A" is the sex tendency of the father. If the father has a male sex tendency, his sons will have a female tendency and his daughters a male tendency. If, on the contrary, the father possesses a female tendency, his sons will have male tendencies and his daughters female tendencies. In other words, sons exhibit the opposite and the daughters the same tendency as the father.

The second factor "B" is the sex tendency of the mother. A mother with a male tendency gives her daughters a female and her sons a male tendency. The mother with a female tendency gives her daughters a male and her sons a female tendency. Thus the transmission of the sex tendency from the female is also criss-cross in the same fashion as that of the male. The females inherit like tendencies from their father and the males like tendencies from their mother, whereas the males inherit the reverse tendency of their father and the females the reverse tendency of their mother.

The third factor "C" is confined to the female and is a change of sex tendency from litter to litter. This change in tendency manifests itself in the following way:

If the first litter contains only males, the mother acquires a female tendency for the next litter and vice-versa. This new tendency varies in strength, depending upon the number of young of one sex contained in a litter. The greater the number of males in a litter, the stronger the female tendency will be for the next litter. This tendency is still more emphasized if the mother is successively mated with males of a definite tendency, and therefore forced to produce more and more young of one sex.

The tendency of the various animals of a certain stock must first be ascertained in an experimental manner; given a number of undetermined males and females, each male must be mated with all the females and each female with all the males. After all the animals have been tested in this way, the results will show more males from some animals and more females from others. If, now, the offspring from these matings be grouped so as to take those animals which have come from more male producing fathers and their tendency be tested, it will be found that from the males more females will be produced and from the females more males. Provided the determination of the sex tendency for the first set of animals was absolutely correct, and if there was no other factor in action, the proportion of males to females should be as 75:25 from male producing males mated with females having different tendencies, and from female producing males the proportion is reversed. It is, however, very difficult to determine absolutely the sex tendency of an animal after only a few matings, and for this reason, some animals supposed to have a male tendency will probably have a weak female tendency, and *vice versa*.

In order to find with reasonable definiteness

the tendency of animals, the tendency of the ancestors for three or more generations should be known. It must also be recalled, as explained above, that the third factor "C" in the female reduces the difference between the ratio numbers of her male and female descendants. As a result of this, the difference between the number of males and females considering only mother tendency is smaller than the difference between males and females in the light of only the father tendency. A male has a male tendency or a female tendency and always maintains it, whereas the female has a born male or female tendency, but in addition to this she has a second tendency to change her sex tendency from litter to litter. The number of males and females derived from the second generation was as follows:

I. Descendants of males whose fathers had a male tendency mated with mixed females; 39 males to 54 females, *i. e.*, 41.94 per cent. males to 58.06 per cent. females (sex ratio 72.20).

II. Descendants of males whose fathers had a female tendency mated with mixed females; 42 males to 23 females, *i. e.*, 64.61 per cent. males to 35.39 per cent. females (sex ratio 182.60).

III. Descendants of males whose mothers had a male tendency mated with mixed females; 64 males to 53 females, *i. e.*, 54.70 per cent. males to 45.30 per cent. females (sex ratio 120.83).

IV. Descendants of males whose mothers had a female tendency mated with mixed females; 12 males to 13 females, *i. e.*, 48 per cent. males to 52 per cent. females (sex ratio 92.30).

V. Descendants of females whose mothers had a male tendency mated with mixed males; 13 males to 18 females, *i. e.*, 41.94 per cent. males to 58.06 per cent. females (sex ratio 72.22).

VI. Descendants of females whose mothers had a female tendency mated with mixed males; 51 males to 43 females, *i. e.*, 54.25 per cent. males to 45.75 per cent. females (sex ratio 118.60).

VII. Descendants of females whose fathers had a male tendency mated with mixed males; 38 males to 36 females, *i. e.*, 51.38 per cent. males to 48.62 per cent. females (sex ratio 105.55).

VIII. Descendants of females whose fathers had a female tendency mated with mixed males; 33 males to 37 females, *i. e.*, 47.15 per cent. males to 52.85 per cent. females (sex ratio 89.18).

These figures show that in the sons the tendency received from the father is stronger than that coming from the mother, while in the daughters the opposite is true.

When one examines the descendants of animals whose fathers had a male tendency and mothers a female tendency, a higher difference in the relative number of males and females is found than from those cases in which the fathers alone had a male tendency. Twenty descendants of such male animals (father male tendency and mother female tendency) mated with mixed females consisted of four males and sixteen females, *i. e.*, 20 per cent. males and 80 per cent. females (sex ratio 25.00). From the females of the same type (father male tendency and mother female tendency) mated with mixed males, 29 males and 15 females were derived, *i. e.*, 65.90 per cent. males to 34.10 per cent. females (sex ratio 193.33).

From the second-generation males whose fathers had a female tendency and whose mothers show a male tendency when mated with mixed females were derived 32 males and 15 females, *i. e.*, 68.08 per cent. males to 31.92 per cent. females (sex ratio 213.33). From the females of the same type (father female tendency and mother male tendency) mated with mixed males were derived 6 males and 13 female descendants, *i. e.*, 31.58 per cent. males to 68.42 per cent. females (sex ratio 46.15).

Should one select males whose fathers had a female tendency and whose mothers had a male tendency and mate these with females whose fathers had a male tendency and whose mothers had a female tendency, a higher difference in the relative number of males and

females will be found in their descendants than in any other possible case. From such combinations were derived nine animals, all males. In the same way, from the combination a male derived from a father, male tendency, and mother, female tendency, mated with a female from father, female tendency and mother male tendency, were derived six animals all of which were females.

This regulation in the inheritance of the sex tendency is especially interesting in affording an explanation of the manner in which the equilibrium is maintained between the number of male and female offspring of a given species. With each new generation each male animal has an opposite tendency from that of his father and each female animal an opposite tendency from that of her mother. It, therefore, follows that a disturbance of the equilibrium in one generation will tend to be restored by the opposite tendencies in the following generation. The above-mentioned change of sex tendency from litter to litter in the female leads to the same result. This third factor "C" regulates equilibrium from birth to birth so that any disturbance of a great degree is impossible.

The difference in the proportion between the sexes in different species may be due to the fact that in some species the father and mother have an equal influence on the determination of the sex of their offspring while in other species either the father or the mother may have the greater influence.

When father and mother have equal influence, the combination father, male tendency, with mother, female tendency, will give equal number males with females tendencies and females with male tendency, and the combination father, female tendency, with mother, male tendency, will give equal number males with male tendency and females with female tendency. In this way equal numbers of male and female descendants will be produced and equal numbers of the descendants will have male and female tendencies. In such a case the sex ratio should be 100 per cent. Should, on the other hand, the mother have the greater influence on the determination of sex, as seems to be the case in the guinea-pigs, then the number of descendants with a male tendency will be greater than the number of those having a female tendency, as the following scheme shows:

Father with male tendency mated with mother with female tendency will give more females (male tendency) than males (female tendency).

Father with a female tendency mated with a mother with a male tendency gives more males (male tendency) than females (female tendency). Therefore from either combination the greater number of offspring have a male tendency and as a result of this the sex ratio will be greater than 100.

In guinea-pigs it really seems that the influence of the mother is greater than that of the father, and this may be the explanation of the fact that the number of male guinea-pigs is greater than the number of females.

Finally, if the father has a greater influence on sex determination than the mother the number of descendants with a female tendency will be greater than the number with a male tendency, and consequently the sex ratio will be smaller than 100, as is shown by the following analysis:

Father with male tendency mated with mother with female tendency gives more males (female tendency) than females (male tendency). The father with female tendency mated with mother with male tendency gives more females (female tendency) than males (male tendency). Thus from either combination the greater number of offspring have a female-producing tendency and as a result of this the sex ratio will be less than 100.

Concerning the third factor, "C," the change of the sex tendency from litter to litter, a statistical examination shows the following results:

First: Relative number of male and female descendants after the birth of one or more females in one litter; 38 males to 12 females, *i. e.*, 76 per cent. males to 24 per cent. females (sex ratio 316.66).

Second: Relative number of male and female descendants after birth of one or more

males; 13 males to 38 females, *i. e.*, 25.49 per cent. males to 74.51 per cent. females (sex ratio 34.21).

To determine further whether the sex-tendency of males showed any inclination to change from mating to mating the records were counted in the following manner:

Taking the matings of given males following matings that produced only female young, it was found that the product of such matings consisted of 22 males to 27 females, *i. e.*, 44.90 per cent. males to 55.10 per cent. females (sex ratio 81.48).

Taking the matings of given males following matings that produced only male young, it was found that the product of such matings consisted of 28 males to 24 females, *i. e.*, 53.84 per cent. males to 46.16 per cent. females (sex ratio 116.66).

This result is therefore the reverse of that shown by the females. Whereas the females show an opposite tendency following each litter, the males always maintain the same tendency.

Only those litters which were purely male or female were used in the above consideration. After a mixed litter of males and females, which is more common under natural conditions, there is not a pure, but also a mixed sex tendency. This fact renders the recognition of the "C" factor extremely difficult.

Such a characteristic change in tendency from birth to birth also seems to occur in other animals. The daphnids, for instance, seem to have some such regulation very definitely expressed.[1] In these organisms also the sex tendency changes from generation to generation as well as from birth to birth in such a way that not after each generation and each birth, but after a number of generations and births, differing with different species, the exclusive production of parthenogenetic female ceases and the first males appear. Doubtless we have in this an example of a change of the sex tendency, but its expression is quite different from that in the guinea-pigs.

[1] Papanicolaou, G., "Experimentelle Untersuchungen über die Fortpflanzungsverhältnisse der Daphniden," *Biol. Zentralbl.*, 30, 1910.

From a theoretical standpoint, it is very important that coincidentally with the change of sex tendency in the summer eggs from female to male in *Moina rectirostris* var. *Lilljeborgii*, there is also a change in the color of these eggs from violet to blue.[2] This fact probably indicates that some chemical change occurs in the eggs at the same time that the change in the sex tendency takes place.

At the present time I am endeavoring to complete my observations and to determine statistically the relative value of the three factors in different combinations. Since, however, the animals at my disposal are designed especially for the study of the degenerative influence of alcohol, it will, no doubt, require a long period of time to collect hundreds of selected cases, since so few animals of the generations later than the third are capable of reproduction.

This preliminary report is published with the hope that other investigators, having a large stock of different animals at their disposal, may further contribute to the solution of this problem in all its details.

George Papanicolaou

Department of Anatomy,
Cornell University Medical College

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SECTION C

The first session was held on the afternoon of Thursday, December 31, at the John Harrison Laboratory of Chemistry, Vice-president Edgar F. Smith in the chair, with an attendance of about 75. The following officers were elected:

Vice-president—William McPherson, Ohio State University.

Member of Council—W. T. Taggart, University of Pennsylvania.

Member of General Committee—L. W. Jones, University of Cincinnati.

Member of Sectional Committee—E. C. Franklin, Stanford University.

The section passed a resolution to the following effect: That the committee of Section C endeavor

[2] *Ibid.*

to arrange its program so as to center round some definite topic or group of topics, the choice and treatment of which should preferably be such that they prove interesting and useful, even to any one not especially conversant with chemistry; and that Section C favors the relegation to the Chemical Society of unrelated papers of interest only to chemists, though this would in no wise preclude the holding of joint meetings with a local section of the Chemical Society or with any other society.

The following papers were read:

The Densities and Degrees of Dissociation of the Saturated Vapors of the Ammonium Halides:[1] ALEXANDER SMITH AND ROBERT H. LOMBARD.

The Entropy of Vaporization of Normal Liquids: J. H. HILDERBRAND.

A discussion of the value of the quotient Q/T (Q is the latent heat, T the temperature, of vaporization) and of its variations.

Chemical Preservation of Manure: P. A. MAIGNEN.

A plea for the better conservation of the valuable material at present going largely to waste as sewage.

A Rapid Lime Requirement Method for Soils without the Use of a Factor: THOS. F. MANNS.

A description of the method, illustrated by examples of the results attained.

On the Universal Application of the Molecular Theory. A Question: H. E. MORROW.

Suggests the possibility of dispensing with the conception of molecules in the case of complex colloids, such as proteins; that a conception of continuous atomic linkings serves to account for some of the properties which such complex substances exhibit.

On the forenoon of January 1, in the laboratory of hygiene of the University of Pennsylvania, Section C held a joint session with Section K and with the Society of American Bacteriologists, devoted to a symposium on "The Lower Organisms in Relation to Man's Welfare." The attendance was about 200. The list of speakers and titles follows; the papers will be published in full later.

Theories of Fermentation: C. L. ALSBERG.

The general mechanism of the action of ferments.

Enzyme Action: C. S. HUDSON.

A discussion of the chemical changes involved in the action of enzymes.

Rôle of Microorganisms in the Intestinal Canal: A. I. KENDALL.

Microorganisms in their Application to Agriculture: C. E. MARSHALL.

JOHN JOHNSTON,
Secretary of Section C

[1] See *J. Am. Chem. Soc.*, 37: 38, 1915.

THE FEDERATION OF AMERICAN SOCIETIES FOR EXPERIMENTAL BIOLOGY

THE second annual meeting of the Federation, comprising the American Physiological Society, the American Society of Biological Chemists, the American Society for Pharmacology and Experimental Therapeutics, and the American Society for Experimental Pathology, was held at St. Louis on December 28, 29 and 30, 1914, in the laboratories of the Washington University Medical School.

Three joint sessions of all of the above societies were held at which twenty-eight communications were presented. The titles of these papers have already appeared in the account of the meetings of the Physiological Society.[1]

At the first session the following memorial addresses were delivered:

"S. Weir Mitchell," by E. T. Reichert, read by W. B. Cannon.

"C. S. Minot," by F. S. Lee.

The following resolution was presented and unanimously adopted:

WHEREAS, various of the European nations with which many of our members are related by birth, descent or intellectual friendship are now at war,

Resolved, that we extend to the scientific men within these nations the hope of an early and enduring peace, which will leave the nations with no permanent cause of rancor towards each other, and which will insure to each the glories of scientific and humanitarian achievement in accordance with its own conception of these ideals.

Printed copies of this resolution, suitable for mailing, have been prepared and may be obtained from Professor Graham Lusk, Cornell Medical College, New York City. It is hoped that members of the Federation will send such copies with their compliments to their scientific friends in the countries now at war.

Executive Committee for the Year 1915.—Chairman, Torald Sollmann; *Secretary,* John Auer, for the Pharmacological Society; W. B. Cannon, C. W. Greene, the Physiological Society; Walter Jones, P. A. Shaffer, the Biochemical Society; Theobald Smith, Peyton Rous, the Pathological Society.

P. A. SHAFFER,
Secretary of the Executive Committee, 1915
WASHINGTON UNIVERSITY MEDICAL SCHOOL,
February 9, 1915

[1] SCIENCE, January 22, 1915, p. 142.

THE AMERICAN SOCIETY OF BIOLOGICAL CHEMISTS

The ninth annual meeting of the society was held at St. Louis on December 28, 29, 30, 1914, in the laboratories of the Washington University Medical School. Three joint sessions were held with the other societies composing the federation in addition to two sessions conducted independently.

The following communications were presented at the independent sessions of the society and from this society at the joint sessions. The titles of other papers which were presented from the other societies at the joint sessions have been given in the account of the meetings of the Physiological Society.[1]

"The Influence of Food on Metabolism," presidential address, by Graham Lusk.

"The Excretion of Creatine During Fasting," by F. D. Zeman (by invitation) and P. E. Howe.

"The Determination of Creatine and Creatinine in Urine; and the Occurrence of Creatine," by J. L. Morris (by invitation).

"A Method for Determining and Comparing the Local Toxicity of Chemical Compounds," by H. J. Corper.

"Experiments with Pure d-l-Glyceric Aldehyde," by R. T. Woodyatt.

"The Level of Sugar in the Blood Flowing from the Liver under Laboratory Conditions," by J. J. R. Macleod and R. G. Pearce.

"The Level of Blood-sugar in the Dog," by P. A. Shaffer and R. S. Hubbard (by invitation).

"A Method for the Decomposition of the Proteins of the Thyroid with a Description of Certain Constituents," by E. C. Kendall.

"Variations in Factors Associated with Acidity of Human Urine During a Seven-day Fast and During the Subsequent Non-protein and Normal Feeding Periods," by F. D. Zeman (by invitation), Jerome Kohn (by invitation) and P. E. Howe.

"The Mechanism of the Toxicity of Halogen Narcotics," by E. A. Graham (by invitation).

"On the Relation of the Oxygen Tension of the Atmosphere to Combustion," by H. C. Dollwig (by invitation), A. C. Kolls (by invitation) and A. S. Loevenhart.

"The Influence of Sodium Carbonate on the Glycosuria, Hyperglycæmia and the Respiratory Metabolism of Depancreatized Dogs," by J. R. Murlin and B. Kramer (by invitation).

"The Possibility that Some of the Hepatic Glycogen May Become Converted into other Substances than Dextrose," by J. J. R. Macleod.

"Narcotics in Phlorhizin Diabetes," by R. T. Woodyatt.

"Some Studies in Autolysis," by H. C. Bradley.

"On the Nature of the Hepatic Fatty Infiltration in Late Pregnancy and Early Lactation," by V. H. Mottram (by invitation).

"The Synthesis of Hippuric Acid in Experimental Tartrate Nephritis in the Rabbit," by F. B. Kingsbury (by invitation) and E. T. Bell (by invitation).

"The Determination of Blood Sugar" (demonstration), by P. A. Shaffer.

New Members—Olaf Bergeim, Jefferson Medical College, Philadelphia, Pa.; Alex. T. Cameron, University of Manitoba, Winnipeg, Canada; G. H. A. Clowes, Gratwick Laboratory, Buffalo, N. Y.; B. M. Duggar, Missouri Botanical Garden, St. Louis, Mo.; Cyrus H. Fiske, Harvard Medical School, Boston, Mass.; R. A. Hall, University of Minnesota, Minneapolis, Minn.; C. G. Imrie, University of Toronto, Toronto, Canada; Benjamin Kramer, State University of Iowa, Iowa City, Ia.; A. Bruce Macallum, University of Toronto, Toronto, Canada; J. F. McClendon, University of Minnesota, Minneapolis, Minn.; J. Lucien Morris, Washington University Medical School, St. Louis, Mo.; Max Morse, University of Wisconsin, Madison, Wis.; V. H. Mottram, McGill University, Montreal, Canada; C. F. Nelson, University of Kansas, Lawrence, Kansas; E. L. Ross, Northwestern University Medical School, Chicago, Ill.; E. C. Shorey, U. S. Department of Agriculture, Washington, D. C.

Officers Elected—The following officers were elected for the year 1915:

President—Walter Jones.

Vice-president—Carl L. Alsberg.

Secretary—P. A. Shaffer.

Treasurer—D. D. Van Slyke.

Additional Members of the Council—Otto Folin, Graham Lusk, L. B. Mendel.

Nominating Committee—J. J. Abel, S. R. Benedict, H. D. Dakin, P. B. Hawk, J. J. R. Macleod, E. V. McCollum, V. C. Myers, T. B. Osborne, A. N. Richards.

Abstracts of the papers presented will be published in the *Journal of Biological Chemistry*.

P. A. Shaffer,
Secretary
Washington University Medical School,
St. Louis, Mo.

[1] Science, January 22, 1915, p. 142.